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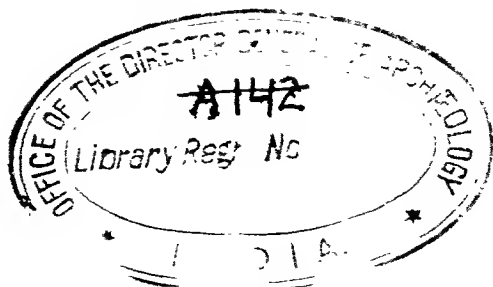
VOLUME LXXIV

1934



PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY

1935



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CONTENTS

	PAGE
List of Illustrations	v
Minutes	vii
Some Governmental Aspects of Regional Planning. GEORGE L. RADCLIFFE	I
Æsthetic and Sociological Aspects of City and Regional Plan- ning. JACQUES GRÈBER	15
The Economic Implications of National Planning. FREDERIC A. DELANO	21
Stabilization of the Currency. RAY B. WESTERFIELD	29
Factors Controlling Prices. Domestic and International ERNEST MINOR PATTERSON	63
Economic Functions of the Stock Market as a Business Agency and the Proposed National Securities Exchange Act of 1934. S. S. HUEBNER.	77
Liberty, Property and Recovery. JAMES T. YOUNG.	93
Problems of Government Ownership and Operation of the Rail- roads. EMORY R. JOHNSON	103
Some Subjective Factors Affecting Industrial Recovery. SAMUEL PRICE WETHERILL	129
R. A. F. Penrose, Jr., Memorial Lecture: A Generation's Progress in the Study of Evolution. EDWIN G. CONKLIN . .	137
The Action of Anæsthetics on Living Protoplasm. L. V. HEILBRUNN	159
The Long Voyages of the Polynesians. ROLAND B. DIXON . .	167
Neurohumors as Activating Agents for Fish Melanophores. G. H. PARKER	177
New Light on Prehistoric Man in Asia. GEORGE GRANT MACCURDY	185
Prochromosomes and Chromosome Structure in <i>Impatiens</i> . FRANK H. SMITH	193
Some Aspects of Genetic Constitution in Relation to Pathology. JOHN W. GOWEN	215
The Linguistic Atlas of New England. HANS KURATH . . .	227
What is <i>Oenothera Hookeri</i> Torrey and Gray. BRADLEY MOORE DAVIS	245

Normal Variation. BY ALEŠ HRDLIČKA	253
The Genera <i>Cordyceps</i> and <i>Ophiocordyceps</i> in Michigan. EDWIN B. MAINS	263
The Thirty-nine Distinct Lines of Proboscidean Descent, and their Migration into all parts of the World Except Aus- tralia. HENRY FAIRFIELD OSBORN	273
A Revision of the American Apatemyidæ and the Description of a New Genus, <i>Sinclairiella</i> , from the White River Oligocene of South Dakota. GLENN L. JEPSEN	287
The Petrography of Some Rocks from South Victoria Land. DUNCAN STEWART, JR.	307
The University of Michigan Collections of Antarctic Rocks and Minerals. DUNCAN STEWART, JR.	311
Index	319

LIST OF ILLUSTRATIONS

NEUROHUMORS AS ACTIVATING AGENTS FOR FISH MELANOPHORES (Parker)

	PAGE
Pl. I, Fig. 1. Melanophore from a Perch	facing 184
Fig. 2. Tail-fin of <i>Fundulus</i>	facing 184
Pls. II-IV. Tail-fin of <i>Ameiurus</i>	following 184
Pls. V-VII. Melanophores	following 184

NEW LIGHT ON PREHISTORIC MAN IN ASIA (MacCurdy)

Pl. I. Mugharet et-Tabun.	following 191
Pl. II. Section of the Three Wady al-Mughara Caves	following 191
Pl. III. Frontal Bone from Tabun Cave	following 191
Pls. IV-V. Lower Jaws from Tabun Cave	following 191
Pl. VI. Upper Arm Bone from Tabun Cave	following 191

PROCHROMOSOMES AND CHROMOSOME STRUCTURE IN *Impatiens* (Smith)

Pl. I, Figs. 1-19. Nuclei or Chromosomes from the Root Tips of <i>I. Balsamina</i>	facing 210
Pl. II, Figs. 20-39. Stages in the Meiotic Divisions of <i>I.</i> <i>Balsamina</i>	facing 212
Pl. III, Figs. 40-53. Chromosomes of Other Species of <i>Impatiens</i>	facing 214

SOME ASPECTS OF GENETIC CONSTITUTION IN RELATION TO PATHOLOGY (Gorren)

Fig. 1. Effects of Focal Melanosis Gene.	facing 216
Fig. 2. Chromosomes of <i>Drosophila</i>	218
Fig. 3. Sperm of <i>Drosophila</i>	219
Fig. 4. Survivorship Curves of <i>Drosophila</i> Exposed to X-ray	220
Fig. 5. Rate of Recessive Sex-linked Lethal Mutations on Exposure to X-ray	221
Fig. 6. Frequency Distribution Showing the Sample of Genes and the Proportions of Them Which Are Completely Lethal	223

THE LINGUISTIC ATLAS OF NEW ENGLAND (*Kurath*)

Maps 1-8. Areas of Settlement and Communities Investigated. Linguistic Maps of New England	229-242
--	---------

WHAT IS *ÆNOTHERA HOOKERI* TORREY AND GRAY (*Davis*)

Pl. I. <i>Ænothera Hookeri</i> Torrey and Gray	following 251
--	---------------

THE GENERA *Cordyceps* AND *Ophiocordyceps* IN MICHIGAN (*Mains*)

Pl. I. <i>Cordyceps michiganensis</i> n. sp.	following 271
Pl. II. <i>Cordyceps superficialis</i> and <i>stylophora</i>	following 271
Pl. III. <i>Cordyceps gracilis</i> , <i>entomorrhiza</i> and <i>Ophiocordyceps</i> <i>clavulata</i>	following 271
Pl. IV. <i>Ophiocordyceps macularis</i> n. sp.	following 271

THE THIRTY-NINE DISTINCT LINES OF PROBOSCIDEAN DESCENT
AND THEIR MIGRATION INTO ALL PARTS OF THE
WORLD EXCEPT AUSTRALIA (*Osborn*)

Fig. 1. Ascent of the Proboscidea	274
Fig. 2. Adaptive Radiation from Genus <i>Elephas</i>	275
Fig. 3. Adaptive Radiation of Generic Phyla of Proboscidea	facing 276
Fig. 4. Elephantine Ridge Crests in <i>Archidiskodon</i>	284

A REVISION OF THE AMERICAN APATEMYIDÆ AND THE DESCRIPTION
OF A NEW GENUS, *Sinclairiella*, FROM THE WHITE RIVER
OLIGOCENE OF SOUTH DAKOTA (*Jepsen*)

Figs. 1-3. Outline of <i>Sinclairiella</i> Tooth	291-2-5
Fig. 4. Outlines of American Apatemyid Lower Jaws	facing 297
Pls. I-III. <i>Sinclairiella dakotensis</i>	following 305

ABSTRACTS FROM THE MINUTES OF
THE MEETINGS

DURING 1934

Stated Meeting, January 5, 1934

ROLAND S. MORRIS, LL.B., LL.D., D.C.L., L.H.D.,

President, in the Chair.

A letter was received from The Historical Society of Pennsylvania thanking the Society for presenting Volume 3 of "Selections from the correspondence of Honourable James Logan" to its Library.

The decease of the following members was announced:

Arthur P. Davis, Sc.D., at Oakland, California, August 7, 1933, æt. 72.

Alfred F. Hess, M.D., Sc.D., at New York, N. Y., December 5, 1933, æt. 58.

William K. Gregory, Ph.D., Professor of Palæontology, Columbia University, and Curator of Comparative Anatomy and Ichthyology at the American Museum of Natural History read a paper on "A Half-Century of Trituberculy, the Cope-Osborn Theory of Dental Evolution." The paper was discussed by Dr. Schaeffer and a guest.

Stated Meeting, February 2, 1934

ALBA B. JOHNSON, LL.D., Vice-president, in the Chair.

Francis R. Packard, recently elected member, subscribed the Laws and was admitted into the Society.

The following resolution was adopted by the Society of American Bacteriologists at its 35th Annual Meeting held at Philadelphia, on December 29, 1933:

As the thirty-fifth annual meeting of the Society of American Bacteriologists draws to a close in the friendly city of Philadelphia, the members record their gratitude to all who have contributed to their comfort and entertainment, and who have so admirably affected the arrangements for their scientific sessions.

The Society instructs its Secretary to transmit its especial thanks to the American Philosophical Society for courtesies extended.

Ernest W. Brown, Sc.D., Professor of Mathematics, Yale University, read a paper on "Time and its Determination." The paper was discussed by Dr. Miller.

Pending nominations were read.

Stated Meeting, March 2, 1934

ROLAND S. MORRIS, LL.B., LL.D., D. C. L., L.H.D.,
President, in the Chair.

Marshall S. Morgan, recently elected member, subscribed the Laws and was admitted into the Society.

The decease of the following member was announced:

William M. Davis, Sc.D., Ph.D., at Pasadena, California, February 5, 1934, æt. 83.

John C. Merriam, Ph.D., Sc.D., LL.D., President, Carnegie Institution of Washington read a paper on "Conservation and Evolution in a Changing Social Program." The paper was discussed by Messrs. Sioussat, Leland, Aydelotte, E. M. Patterson and Emory R. Johnson.

The Committee on Nominations made its Report, (Cyrus Adler, Chairman).

The following resolution was adopted:

RESOLVED: THAT The draft of the Revision of the Laws of the American Philosophical Society in ten chapters, as prepared by the Committee on the Revision of the Laws in substitution for the present Laws, be and the same is hereby proposed to the Society at its Stated Meeting on the 2nd of March, 1934, to be laid before the Society at its next General Meeting in April, 1934, after due notice to the members as

provided in the Charter and Laws, for such action as the Society may be pleased to take thereon.

The new Laws as proposed will be found in original Minutes.

Annual General Meeting, April 19, 20, 21, 1934

Thursday Morning, April 19

Executive Session, 9:30 o'clock

ROLAND S. MORRIS, LL.B., LL.D., D.C.L., L.H.D.,
President, in the Chair.

Burton E. Livingston and Henry A. Sanders, recently elected members, subscribed the Laws and were admitted into the Society.

The decease of the following members was announced:

Augustus Trowbridge, Sc.D., Ph.D., at Sicily, Italy,
March 14, 1934, æt. 66.

George Owen Squier, Ph.D., D.Sc., at Washington, D. C.
March 24, 1934, æt. 65.

Archibald B. Macallum, M.D., Ph.D., Sc.D., LL.D.,
F.R.S., in Canada, April 5, 1934.

Dr. Leland, Chairman, Committee on Revision of Laws, presented a copy of the proposed revision with suggestions and corrections submitted to date.

This revision was previously discussed by the Society sitting as a Committee of the Whole.

The Committee's Report was accepted and the Revision of Laws was referred to Council and then again to the Committee on Revision of Laws.

Professor Scott reported for the Committee appointed to consider what proportion of members should be from Philadelphia.

Morning Session, 10:30 o'clock

ROLAND S. MORRIS, LL.B., LL.D., D.C.L., L.H.D.,
President, in the Chair.

The following papers were read:

- “Cyto-taxonomic Studies on Certain *Oenotheras* from California,” Ralph E. Cleland, Professor of Biology, Goucher College.
- “What Is *Oenothera Hookeri*, Torrey and Gray?” Bradley M. Davis, Professor of Botany, University of Michigan.
- “New Amplifications of the North American Piperaceæ,” William Trelease, Professor Emeritus, University of Illinois. (Read by title.)
- “Some Physico-chemical Properties of the Virus of Typical Tobacco Mosaic,” B. M. Duggar, Professor of Physiological and Economic Botany, University of Wisconsin.
- “New Developments in Automatic Control of Soil Moisture in Plant Cultures,” Burton E. Livingston, Director, Laboratory of Plant Physiology, The Johns Hopkins University.
- “The Action of Anæsthetics on Living Protoplasm,” Lewis V. Heilbrunn, Associate Professor of Zoology, University of Pennsylvania. (Introduced by Dr. Calvert.)
- “Evidence for a Concentrating Neurohumor in the Responses of Fish Melanophores,” G. H. Parker, Director, Zoological Laboratories and Professor of Zoology, Harvard University.
- “The Properties of Catalytic Surfaces as Revealed by Heavy Hydrogen,” Hugh S. Taylor, Professor of Chemistry, Princeton University.

Afternoon Session, 2 o'clock

ROLAND S. MORRIS, LL.B., LL.D., D.C.L., L.H.D.,
President, in the Chair.

The following papers were read:

- "A Spectral Survey of the Nearest Galaxy, the Large Magellanic Cloud," Annie J. Cannon, Curator, Harvard College Observatory.
- "Molecules in Stellar Atmospheres." Henry Norris Russell, Professor of Astronomy and Director of the Observatory, Princeton University.
- "Nuclear Physics at the Bartol Research Foundation of the Franklin Institute," W. F. G. Swann, Director, Bartol Research Foundation.
- "An Apatemyid from the White River Oligocene of South Dakota," Glenn L. Jepsen, Princeton University. (Introduced by Dr. Sinclair.)
- "Geomorphic Investigations in the Yellowstone Park and Big Horn Regions of Wyoming," Douglas Johnson, Professor of Physiography, Columbia University.
- "The Report on the White River Fauna," William B. Scott, Professor Emeritus, Princeton University.
- "New Fishes from the Triassic of Pennsylvania," W. L. Bryant, Director, Park Museum. (Introduced by Dr. Sinclair.) (Read by title.)
- "Solar Radiation, Lightning, the Azotobacter and Proto-plasm," George Crile, Director, Cleveland Clinic and of the Cleveland Clinic Hospital.
- "Some Comparative Results with a New Semi-automatic Respiration Calorimeter," John R. Murlin, Department of Vital Economics, University of Rochester.
- "A Simple, Rapid Basal Metabolism Measurement for Hospital Routine," Francis G. Benedict, Director, Nutrition Laboratory, Carnegie Institution of Washington.

Friday Morning, April 20

Executive Session, 10 o'clock

ROLAND S. MORRIS, LL.B., LL.D., D.C.L., L.H.D.,
President, in the Chair.

Gilbert Chinard, William H. Collins, William B. Dinsmoor, Edward V. Huntington, Charles A. Kofoed, Lewis R. Jones and John R. Murlin, recently elected members, subscribed the Laws and were admitted into the Society.

The President presented his report concerning the state of the Society's business during 1933 and 1934.

The proceedings of the Council were submitted and on motion approved.

Dr. Conklin speaking for the Committee on Grants stated that the Committee was formerly named "Committee on the Use of Funds for the Advancement of Knowledge Through Investigation" but that it was now generally known as the Committee on Grants. The Committee consists of the following members:

Edwin G. Conklin,
John A. Miller,
James T. Young,
Karl T. Compton,
Roland S. Morris.

Dr. Conklin read the Committee's general principles and the list of grants made to date. He then stated that the Committee was completely out of funds although there were still many worthy applications before it and requested an extra appropriation for 1934.

This was unanimously approved by the Society subject to the approval of the Committee on Finance.

The President requested the Committee on Use of Funds to handle both the Magellanic and Phillips Prize awards. This and other suggestions made by the President were on motion approved.

It was the sense of the meeting that two general meetings be held each year.

The Society proceeded to an election.

The tellers subsequently reported that the following officers and members had been duly elected:

President

Roland S. Morris

Vice-presidents

Edwin G. Conklin

Alba B. Johnson

Robert A. Millikan

Secretaries

Arthur W. Goodspeed

John A. Miller

Curator

Albert P. Brubaker

Councillors

(To serve for three years)

Charles G. Abbot

John Cadwalader

James A. Montgomery

Hugh S. Taylor

Members

Detlev W. Bronk

Willa Cather

Gustavus Wynne Cook

Wilbur L. Cross

Cass Gilbert

Edward S. Harkness

Horace Howard Furness Jayne

Alfred Vincent Kidder

John Livingston Lowes

Frederick Novy

Conyers Read

Jesse S. Reeves
Owen J. Roberts
George Sarton
Deems Taylor

Afternoon Session, 2 o'clock

ALBA B. JOHNSON, LL.D., Vice-president, in the Chair.

Samuel Price Wetherill, recently elected member, subscribed the Laws and was admitted into the Society.

The following Symposium on Community Planning, introduced by Colonel Wetherill, was presented:

"Some Governmental Aspects of Regional Planning."

George L. Radcliffe, First Vice-president and member, Executive Committee, Fidelity and Deposit Company, Baltimore. Regional adviser of public works.

"Esthetic Aspects of Regional and City Planning," Jacques Grèber, Architect, Institut d'Urbanisme.

"Some of the Economic Implications of National Planning." Frederic A. Delano, President, American Civic Association and Chairman of the National Planning Board. (Read by Charles J. Rhoads.)

Friday Evening, 8 o'clock

THE R. A. F. PENROSE, JR., MEMORIAL LECTURE

Edwin G. Conklin, Professor of Zoology, Princeton University, spoke on "A Generation's Progress in the Study of Evolution."

Saturday Morning, April 21

Morning Session, 10 o'clock

EDWIN G. CONKLIN, Ph.D., Sc.D., LL.D., Vice-president, in the Chair.

The following papers were read:

"Linguistic Atlas of the United States," Hans Kurath, Brown University. (Introduced by Dr. Lingelbach.)

- "The Beatty Papyrus of the Epistles of Paul." Henry A. Sanders, Professor of Latin, University of Michigan.
- "Turkish Remains in Modern Belgrade," John Dyneley Prince, Professor of Slavonic Languages, Columbia University. (Read by title.)
- "The Chalcolithic or Copper-Stone Age in the Near East," W. F. Albright, Professor of Semitics, The Johns Hopkins University.
- "The Long Voyages of the Polynesians," Roland B. Dixon, Professor of Anthropology, Harvard University. (Read by title.)
- "New Light on Prehistoric Man in Asia," George Grant MacCurdy, Research Associate in Prehistoric Archaeology and Curator of the Anthropological Collections, Yale University.
- "Studies in Human Variation." Aleš Hrdlička, Curator, Division of Physical Anthropology, U. S. National Museum, Smithsonian Institution.
- "Some Aspects of the Constitutional Basis of Pathology." John W. Gowen, Rockefeller Institute. (Introduced by Dr. Conklin.)
- "A New Synthesized Pure-breeding Chromosome Type in the Jimson Weed, *Datura Stramonium*," A. F. Blakeslee, Assistant Director in Plant Genetics, Carnegie Station for Experimental Evolution, Cold Spring Harbor, A. G. Avery and A. Dorothy Bergner. (Read by title.)
- "The Thirty-nine Distinct Lines of Proboscidean Descent and Their Migration into all parts of the World Excepting Australia," Henry Fairfield Osborn, American Museum of Natural History. (Résumé presented by Professor Scott.)
- "Prochromosomes and Chromosome Structure in *Impatiens*," Frank H. Smith, University of Michigan. (Introduced by Dr. Bartlett.) (Read by title.)

Afternoon Session, 2 o'clock

ROLAND S. MORRIS, LL.B., LL.D., D.C.L., L.H.D.,
President, in the Chair.

The following Symposium on Problems of Business Recovery was presented:

- "Currency Stabilization," Ray B. Westerfield, Professor of Political Economy, Yale University. (Introduced by Dr. Johnson.)
- "Factors Controlling Prices, Domestic and International," Ernest M. Patterson, Professor of Economics, Wharton School of Finance and Commerce, University of Pennsylvania.
- "Economic Functions of the Stock Market and the National Securities Exchange Act of 1934," S. S. Huebner, Professor of Insurance and Commerce, Wharton School of Finance and Commerce, University of Pennsylvania.
- "Liberty, Property and Recovery," James T. Young, Professor of Public Administration, University of Pennsylvania.
- "Problems of Government Ownership and Operation of Railroads," Emory R. Johnson, Professor of Transportation and Commerce, Wharton School of Finance and Commerce, University of Pennsylvania.
- "Some Subjective Factors Affecting Industrial Recovery," Samuel Price Wetherill, Engineer.

Saturday Evening, 7:30 o'clock

The annual dinner was held at the Bellevue Stratford Hotel. The speakers at the dinner were Roland S. Morris, presiding, Robert A. Millikan and J. Dyneley Prince.

Stated Meeting, November 2, 1934

ROLAND S. MORRIS, LL.B., LL.D., D.C.L., L.H.D.,
President, in the Chair.

Letters accepting membership were read.

As a mark of appreciation of the part taken by the American Philosophical Society in the Lafayette Centenary Celebration, Monsieur Jules Henry on behalf of the French Government presented the Society with a medal bearing the profile of Lafayette drawn by the engraver Gatteaux in 1830.

The following papers were read:

"The Progress of the Pennsylvania Local Government Survey," James T. Young, Professor of Public Administration, University of Pennsylvania.

"Duplicate Tax Assessments," Blake E. Nicholson, University of Pennsylvania.

"Reorganization of the Second Class Township," Edward W. Carter, University of Pennsylvania.

"Administrative Problems of Borough Organizations," Bradford W. West, University of Pennsylvania.

"Reorganization of the First Class Township," Phillip B. Willauer, Ursinus College.

Messrs. Nicholson, Carter, West and Willauer were introduced by Dr. Young.

The following paper was read by title:

"The Genera Cordyceps and Ophiocordyceps in Michigan," Edwin B. Mains, University of Michigan.
(Introduced by Dr. Bartlett.)

Dr. Conklin, Chairman, Committee on Grants, presented the following list of grants made to date:

- No. 1. Thomas C. Poulter, Byrd Antarctic Expedition II
—for construction of echo sounding apparatus for measuring depth of polar ice cap and discovering nature of its basic support.
- No. 2. Horace Elmer Wood, Dana College, Newark, N. J.,

- to assist in the prosecution of his research and publication on the anatomy, stratigraphic distribution and phylogeny of the rhinoceroses and related groups of perissodactyls.
- No. 3. Warren K. Moorhead, Phillips Academy, Andover, Mass., to help finance the work he is undertaking on the Amerinds of New England.
- No. 4. William B. Scott, Princeton, N. J., to enable him to prepare a monograph on the fossil mammals of the White River formation in Dakota and Nebraska.
- No. 5. James T. Young, University of Pennsylvania, Philadelphia, in support of work on a Survey of Local Rural Government in Pennsylvania.
- No. 6. Robert A. Millikan and John P. Buwalda, California Institute of Technology, Pasadena, Calif., in support of work on the determination of the geological time scale in years.
- No. 7. Alfred C. Lane, Barnum Museum, Tufts College, Boston, Mass., in support of a coöperative research in Physics and Chemistry as to the relations of the various radio-active elements and the lead produced therefrom.
- No. 8. Felix E. Schelling for the Modern Language Association, Philadelphia, Pa., for an edition of Richard II and of Henry IV to be prepared on the plan instituted by Dr. Horace Howard Furness in his new Variorum Shakespeare.
- No. 9. W. F. G. Swann, Bartol Research Foundation of the Franklin Institute. Swarthmore, Pa., for investigations in Nuclear Physics.
- No. 10. Academy of Natural Sciences of Philadelphia, Philadelphia, Pa. in support of (1) A study of the distribution, sources and relationships of the Avifauna of Bolivia. (2) To collect and make field studies of plants, especially of the family Scrophulariaceæ, in northern Mexico, from Nuevo Leon to Chihuahua, considering the composition and distribution of the

flora, and its relation to that of the southwestern United States and southern Mexico. (3) To collect and make field studies of mollusks of the northern states of Mexico from Nuevo Leon to Chihuahua, with the object of determining the relations of the Sonoran fauna of our southwest to the Neotropical fauna of Mexico.

- No. 11. P. W. Whiting, Carnegie Institution of Washington, Department of Genetics, Cold Spring Harbor, Long Island, N. Y., to enable him to continue his investigations on genetics and sex-determination of the parasitic wasp *Habrobracon*.
- No. 12. George M. Reed, Brooklyn Botanic Garden, Brooklyn, N. Y., to enable him to carry on his work on the Influence of the nutrition of the host on smut development.
- No. 13. J. Lincoln Cartledge, Carnegie Institution of Washington, Department of Genetics (Laboratory guest), Cold Spring Harbor, Long Island, N. Y., to enable him to continue his investigation of the factors which are responsible for the increased mutation rate in aged seeds of *Datura*; the effects on the mutation rate of temperature, moisture, oxygen supply, and other experimentally applied factors and the effects of aging seeds under natural conditions as when buried in the soil; the study and determination of the changes brought about by these means.
- No. 14. A. V. Grosse, Visiting Assistant Professor, University of Chicago, Chicago, Ill., to enable him to continue his work on the extraction of 1 gram of the radioactive element 91—protactinium—from about 5 tons of raw material and its isolation in the form of pure salts and finally in the metallic state itself.
- No. 15. Edward L. Thorndike, Teachers College, Columbia University, New York, N. Y., in support of a research in the psychology of animal and human learning.

- No. 16. V. M. Slipher for the Lowell Observatory, Flagstaff, Ariz., for extending the search of the ecliptic, covering a wide belt of the sky, for outer members of the solar system, because the small size and faintness of Pluto made it seem not improbable that other similar bodies would be found, and showed that such an exacting and complete search would be required to give answer to this important question.
- No. 17. Edward L. Bowles and Henry G. Houghton, Jr., Massachusetts Institute of Technology, Cambridge, Mass., to cover expenses in connection with the programme of Research on Fog which may be divided into three parts, viz.: (1) An investigation of the transmission of radiation through fog. (2) The development of apparatus for and the measurement of the physical and chemical properties of fog such as the number of particles and their size and the chemical nature of the nuclei of condensation. (3) The development of a method for the local dissipation of fogs.
- No. 18. Arthur J. Dempster, University of Chicago, Chicago, Ill., in support of an investigation on the determination of the exact atomic weight ratios of the chemical elements by the methods of mass spectroscopy.
- No. 19. John R. Murlin, University of Rochester Medical School, Rochester, N. Y., in support of an investigation into the effects of high frequency currents on the energy metabolism of animals and the human subject.
- No. 20. H. H. F. Jayne, for the University Museum, University of Pennsylvania, Philadelphia, Pa., to assist the Museum's work in the Archæological Excavations in the Manich River Valley of the Caucasus together with the State Academy of Leningrad. It was not possible to carry out this project and the grant was returned.
- No. 21. Hellmut de Terra, Peabody Museum of Natural

History, Yale University, New Haven, Conn., to enable him to study the geological background of early man in Northern India through the use of the concerted methods of geology, paleontology and prehistory, and to carry out an organized search for early hominids and fossil anthropoid apes for the sake of the advancement of our knowledge of man's evolution and his earliest cultures.

- No. 22. Ralph E. Cleland, Goucher College, Baltimore, Md., in support of his work for a coöperative cyto-genetic and taxonomic attack upon the phylogeny and systematics of *Oenothera* (evening primrose), with special reference to the sub-genus *Onagra*.
- No. 23. F. K. Richtmyer, Cornell University, Ithaca, N. Y., to enable him to continue his work on the determination of the widths, shapes and relative intensities of the lines in the X-ray spectra of the several elements; and the use of these data to compute the distribution of energy in the excited states of atoms.
- No. 24. Farrington Daniels and B. M. Duggar, University of Wisconsin, Madison, Wis., in support of a fundamental research in photosynthesis, concerned with a determination of the quantum efficiency in this process when employing monochromatic light in different regions of the spectrum using algæ as test material.
- No. 25. K. Lark-Horovitz, Purdue University, Lafayette, Ind., in support of his investigation on the Intensity of Electron Scattering by means of Homeo-polar compounds.
- No. 26. Harry Shultz Vandiver, University of Texas, Austin, Tex., to enable him to continue his work on the computation and investigation of the properties of Bernoulli Numbers with special application to Fermat's Last Theorem, perhaps the best known of all unsolved mathematical problems.
- No. 27. Frank G. Dunnington, California Institute of Tech-

- nology, Pasadena, Calif., to enable him to continue his work on a precision determination of the specific charge of a free electron by a new deflection method.
- No. 28. N. T. Bobrovnikoff, for the Perkins Observatory, Delaware, Ohio, for investigations of stellar spectra, mostly in the red and infra-red, with a special attention to the band spectra.
- No. 29. Judson Daland, Philadelphia Institute for Medical Research, Philadelphia, Pa., in support of work on the biological effects of thymus extract (Hanson); the accruing acceleration in the rate of growth and development in successive generations, from the extract of Thymus.
- No. 30. Frank C. Jordan for the Allegheny Observatory, Pittsburgh, Pa., to cover expenses in connection with the work on the measurement of plates and computations for the determination of stellar parallaxes.

The decease of the following members was announced:

Paul Shorey, LL.D., Litt.D., at Chicago, Ill., April 24, 1934, æt. 77.

William H. Welch, M.D., at Baltimore, Md., April 30, 1934, æt. 84.

John Cadwalader, LL.B., at Philadelphia, Pa., June 10, 1934, æt. 61.

Nathaniel Lord Britton, Ph.D., Sc.D., LL.D., at New York, N. Y., June 25, 1934, æt. 75.

Marie Skłodowska Curie, D.Sc., LL.D., at Paris, July 4, 1934, æt. 66.

Cass Gilbert, LL.D., A.F.D., D.Litt., at Brockenhurst, England, May 17, 1934, æt. 74.

Sir Arthur Schuster, Sc.D., Ph.D., F.R.S., in England, October 14, 1934, æt. 83.

Raymond Poincaré, at Paris, October 15, 1934, æt. 73.

Ramon y Cajal, at Madrid, October 17, 1934, æt. 82.

James C. Wilson, M.D., at Philadelphia, October 28, 1934, æt. 87.

On motion the Secretaries were ordered to prepare a letter

of condolence to the French Ambassador expressing the Society's grief at the death of Raymond Poincaré.

The following resolution was adopted and a copy ordered to be forwarded to Mr. Lewis:

RESOLVED THAT The Society accepts with much appreciation the fine portrait of LOUIS AGASSIZ, a member of the Society from 1843 to 1873, by Daniel Huntington, presented by John F. Lewis, Jr. The Society wishes to express its gratitude to Mr. Lewis.

Stated Meeting, December 7, 1934

ROLAND S. MORRIS, LL.B., LL.D., D.C.L., L.H.D.,
President, in the Chair.

Gustavus Cook, recently elected member, subscribed the Laws and was admitted into the Society.

The decease of the following member was announced:

John M. Gest, LL.B., at Philadelphia, November 30, 1934, æt. 75.

Jotham Johnson, University of Pennsylvania Museum spoke on the Excavations of the University Museum at Minturnæ. The paper was discussed by the President.

Mr. Morris stated that the November 2nd meeting was the first occasion on which a Report was made of a project sponsored by the R. A. F. PENROSE, JR. Fund.

Russell Duane was elected a Councillor to fill the vacancy created by the death of John Cadwalader.

The dates of the Annual General Meeting, April 18, 19, 20, 1935, were formally ratified.

PROCEEDINGS
OF THE
AMERICAN PHILOSOPHICAL SOCIETY
HELD AT PHILADELPHIA
FOR PROMOTING USEFUL KNOWLEDGE

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No. 1

SOME GOVERNMENTAL ASPECTS OF REGIONAL
PLANNING

GEORGE L. RADCLIFFE

(Read April 20, 1934)

GRANTED that a thing either is or is not and that it cannot both be and not be, certainly it is true that efforts to describe what a thing is by telling what it is not is unbusiness-like and tedious. In spite of that fact, I am beginning this talk by utilizing methods of elimination.

Naturally, Governmental planning used in its largest sense concerns necessarily a vast multitude of ideas and activities. I have reference generally to mandates of law and regulations of society. I have in mind, of course, all of the myriad of rules and regulations, legal, physical, economic, financial, industrial, social, theological, etc., which society has set up and adopted for its own.

Were I competent in this paper, which, of course, I am not, to attempt to make even an outline of such regulations, I would not have the time to do so nor would this or any audience listen to me. My present plight suggests a well-known illustration of the make-up of an atom; that is, that the movement of an electron within the atom is somewhat similar to that of a bumble bee flying around the interior of a big railroad station. In this matter I am going to try to be the

bumble bee, but certainly not the railroad station. Doubtless, you will feel that my movements will be about as unaccountable as those of a bumble bee, in or out of a railroad station.

Each one of us during a lifetime collects a curious assortment of hobbies or ambitions. At a certain stage in my youth, I became possessed with the idea that I was going to devote myself to the study of the philosophy of history. I realized that I did not know what it meant but there was something about the name which attracted me. Once I ran into a definition of the philosophy of history to the effect that "The Philosophy of History is nothing more than the thoughtful contemplation of it." That somewhat vague definition had rather a depressing effect upon me. I had believed that the term "philosophy of history" suggested structures more formal and also mechanics of a greater imposing nature than merely contemplation. Of course, it is true that the province of either the philosophy of history or of history itself is now regarded as much more constructive than was suggested by the old-fashioned description of history as merely a record of past events.

Planning from a standpoint of either public or private works is infinitely more than the contemplation and consideration of problems. It involves purposes and execution. Yet don't we often lose sight of the fact, however, in our desire to stress the importance of action, that we have overlooked the necessity for well-ordered thought? Rather curiously we do not seem to be making much progress in *laissez-faire* with the passing of time. The Mosaic Code, the Laws of Lycurgus, Draco, Solon, and even the Institutes of Justinian involved a tremendous amount of constructive planning for people, whether from the standpoint of government or society. Those who wrote about ideal commonwealths laid little stress on the doctrine of rugged individualism. Isn't that evident in More's *Utopia*, Bacon's *Atlantis*, Campanelli's "City of the Sun" and many other similar books?

It is interesting to see, as a matter of fact, what small per-

centages separate the views of the present advocates of the New Deal from those of the so-called rugged individualism or laissez-faire of a few years ago. In other words, if you could estimate the percentage of free-will in our relations with our fellow-men as upheld by the political philosophy dominant in Washington a few years ago, and then contrast that with the views of those who hold the most advanced theories of the New Deal, I wonder if the percentage of difference would not be absurdly small. For instance, let us assume that the advocate of the New Deal believes in 80 per cent of regulation. Those of the older regime would probably reach a percentage of say 75 per cent determinism and 25 per cent free-will. I doubt whether there would be over 5 per cent of difference between the two theories.

Undoubtedly, the cruel rigidity of savage customs and taboos exceed the restraints imposed by modern civilized society. Even the hermit savage was not a free lance. Dangers from wild animals and wilder men, besides terrors of cold and starvation, encompassed his life.

All of this seems to have very little to do with modern concrete questions of planning. Instead of attempting some general synopsis of the subject, which would certainly be hopelessly incomplete and inaccurate, I am going to talk for a few minutes about several aspects of governmental planning with which I have had some contacts recently. I will not trouble you with charts, diagrams and technical language, but my statements will be quite general in character.

Last July I found myself most unexpectedly Regional Adviser of Federal Emergency Administration of Public Works in Region No. 10, consisting of the following states: Maryland, Delaware, Virginia, West Virginia, Tennessee, Kentucky, North Carolina and the District of Columbia. My duties, as set forth briefly in black and white and by conversation, were vague. I found, however, that I was expected to do apparently two things. One was to work out a plan for my region as to federal, state, municipal public works, etc., familiarizing my associates and the public with

that plan. The second was to see that public works were started and carried out in accordance with that plan.

Obviously, the existence of a plan was a prerequisite. I had no plan and the emergency program of public works could not wait for one to be evolved. The result was that we had the cart before the horse. Apparently, the only way to begin was to go ahead with specific public works projects, meanwhile trying to create a plan for them, general in nature. We could, at least, try to keep public works from being started which seemed to be antagonistic to the general ideas of any national plan and develop some projects which would be an obvious contribution to any national plan.

I had the good fortune to secure the services of ten advisers and co-workers, engineers, architects, lawyers, business men, health experts, etc. These men served without compensation and for a long time without any official status in Washington. We met daily.

How could we prepare a plan if we did not know the facts upon which our plan should rest? Our knowledge of such pertinent conditions, as physical, economic, financial, scenic, industrial, in the seven or eight states was entirely inadequate for the preparation of any plan.

Of course, a tremendous amount in the nature of public works was already in existence. I was reminded of a paper I was once called upon to prepare in the graduate school of Johns Hopkins University on Survivals of Rome. What of Rome really survived; what of that was Roman by creation or by adoption, were among the many questions which were relevant. On a tiny scale, there is really a bit of analogy in the present situation.

The character and extent of existing public works in my region was sure to have necessarily a decided effect upon what should be added. All sorts of questions would naturally arise, however, as to how far the theory of the old should be disregarded in the planning for the new. There was, however, the chance of a systematic study of the subject in a manner somewhat different from what had been attempted before.

One of our chief functions was the coördination of our activities with those of existing agencies. Thus our work in Tennessee and some of the adjoining states required us, naturally, to be in close contact with the Tennessee Valley Authority.

I cannot imagine anything which would be more tedious to you than a statement of how we tried to familiarize ourselves with conditions in our region and what progress we made.

The United States Government had already given considerable thought to the problems of planning from a country-wide standpoint, but usually as to matters of federal buildings, harbor and river improvements, etc. Construction of interstate roads was receiving more and more consideration in Washington. Frequently a state in planning for its public works had consulted with an adjoining state where there was a community or clash of interests. The Emergency Public Works Act, however, permitted coöperation between the Federal Government and the state governments on a basis somewhat better than ever before, for instance, in the case of the Shenandoah-Great Smoky Mountain Parkway. The route between these two parks has been selected only in part. It will end at the Great Smoky Mountain on the border line between Tennessee and North Carolina, after beginning at Shenandoah Park in Virginia. Experts state that the parkway, when developed, will from scenic and recreational standpoints be one of the finest in this country, if not in the world. It will be approximately five hundred miles in length.

Since the three states, Virginia, North Carolina and Tennessee, were located in my region, I was asked to be Chairman of the Supervisory Committee, consisting of the heads of the federal departments of parks, roads, forests and certain representatives from the state government from the three states. The basis for federal and state coöperation is easily a workable one. The Federal Government is to supply the most of the money and will really select the route. The various states will contribute by making surveys, securing rights

of ways and in other respects. The arrangement between the Federal Government and the state governments has so far worked satisfactorily.

Doubtless, millions of people will travel over this parkway when it is completed and use its grounds for recreational purposes. That is, however, not all of the story. It is desirable that an adequate system of roads should lead into this parkway from various directions. Consequently, a study of existing roads as well as those which should be built as arterial approaches is involved.

Naturally, economic uses of such a parkway are of a very considerable value. The tourist trade is a cash one. Sales of food, gasoline and various forms of commodities along the parkway will be large. The opportunities for labor will be increased in various ways. In fact, the economic results will spread far into the hinterland.

Some communities in the mountain regions crossed by the parkway are, as we know, in an unfortunate economic plight. Many of those residents are now being supported by federal aid. Some communities have but one basic industry, such as coal. Others have none and agriculture may be on a subsistence basis only. It may be that the coal industry in such a section cannot be successfully stimulated. Either public support of the inhabitants must continue indefinitely or new industries must be found or residents must move to new parts of the country.

Bringing new industries into a community successfully is not easy. There is almost always the danger that the new industry cannot be set up on a basis which is economically sound. "It is hard for an empty bag to stand upright." An industry transplanted arbitrarily and without consideration of its relevant factors is likely to tumble down. All sorts of questions as to raw materials, transportation facilities and rates and industrial methods generally are involved. It is obvious, however, that any wholesale method of installing new industries in impoverished communities is a difficult one to apply successfully. We know that new industries are from

time to time established successfully in new communities, sometimes by transference of existing plants and organizations, often either by the creation of new businesses or by establishing branches of ones existing in other communities.

We underestimate almost invariably the possibilities of shifting population from one place to another. Many people do not want to leave their communities and possibly would be misfits in new localities. Who is to be moved, where are they to be placed, what would be the effects upon the new community as well as the old, are among the numerous questions which press for determination, and yet we know that the idea of movements of people is age-long in history. Wandering people have swept over the earth from the beginning possibly of human life, frequently from Asia, as a starting place. Nomadic people have often conquered and absorbed the people which they overran. I am not going to get into troublesome questions as to how far the conquering hordes of Asia absorbed or were absorbed by the people of Europe whom they overran.

The point is that we have always been accustomed to minimize the difficulties in the way of successful transmigration of people. The wanderings of the Israelites from Egypt to Palestine, the flight of the so-called Tartar Tribe, the disappearance of the ten tribes of Israel, the conquests of Genghis Khan, and Tamberlane do not offer helpful suggestions when we consider possibilities of transferring a part of the population in these days from one section of the United States to another.

A study on a wider basis, I think, than ever before attempted is being made of these questions regarding industry and population. As Regional Adviser of Public Works, I had the opportunity to participate to some extent in these studies, working in the main with certain well-known federal agencies. It is true, indeed, that the Shenandoah and Great Smoky Mountain Parkway planning involves consideration of economic phases of the situation as well as the scenic, recreational and others that readily occur to us. It should,

because of the anticipated large tourist travel, make a major economic contribution to the sections through which it will pass.

Surveys and studies of the Chesapeake Bay Region have been very interesting to us. Some of our approaches to the matter are as follows:

PUBLIC HEALTH

- (1) Pollution of Oyster Beds.
- (2) Pollution of Bathing Beaches.
- (3) Sanitary Sewers,
 - (a) Studies of cities and towns now having or undertaking sewer systems.
 - (b) Studies of cities and towns without and not at present contemplating sewer systems.
 - (c) Studies of cities and towns now having or undertaking disposal plants.
 - (d) Studies of cities and towns without and not at present contemplating disposal plants.
- (4) Water Supply and Purification,
 - (a) Studies of cities and towns having adequate water supply and purification plants.
 - (b) Studies of cities and towns having inadequate water supply and purification plants.

INDUSTRIAL

Oyster, Crab and Fish,

- (a) Show decline in the production of the oyster in Maryland and Virginia as against the increase in competing states.
- (b) Show cause.
- (c) What steps should be taken to increase production?
- (d) Ascertain condition of crab and fish industry in Maryland and Virginia.
- (e) Ascertain markets to which go these seafoods, both raw and packed. Give facts (if obtainable) on decreased demand in these markets and why.
- (f) The importance of fishing and duck shooting by visiting sportsmen as a source of revenue to residents of district.

CONSERVATION

Wild Fowl and Muskrat,

(a) Does income from duck shooting, etc., justify efforts to increase supply of wild fowl?

(b) Should certain areas, such as islands, be purchased and set aside and restricted to use, as shelters, to increase production of wild fowl?

(c) Should replanting of feeding grounds with wild celery, rice, etc., be encouraged?

(d) What steps should be taken to increase and develop the muskrat industry?

PHYSICAL

(1) What effect has shore erosion on oyster beds?

(2) What effect has shore erosion on community developments?

(3) Should increased, artificial or natural, harbor or other shelter accommodations be provided for small pleasure craft (especially along West Shore)?

(4) Are the fines for the discharge, by ships, of bilge oil into bay sufficiently heavy to discourage this practice?

RECREATIONAL

(1) Should the recreational advantages of the area be stimulated?

(2) Should county-owned casinos or club houses, with golf links, water activities, etc., on water front property be suggested?

(3) Should state or county owned and operated rental camps or cabin communities (similar to those in the Canadian Lake Country) at vantage points and at reasonable intervals on bay shores be encouraged?

Roughly speaking, we have had in mind three maps of the Chesapeake Bay and its tributaries. One of the Chesapeake Bay as it is; another of a Chesapeake Bay as a somewhat idealized goal; the third, the Chesapeake Bay, as a practical modern prospect. The third map was the one which recently

received close attention. We have had in that work the very close coöperation of the state, county and municipal authorities in the Chesapeake Bay area and of many departments in Washington. Numerous conferences have been held with health authorities, conservation officials and various state and municipal representatives. I am confident some substantial progress has been made in securing a closer coöperation between the District of Columbia and various states in the Chesapeake Bay Region.

I shall not attempt to refer even to the various projects which were taken up, many of which have been completed successfully or show very definite progress. One encouraging aspect is the fact that it should be somewhat easier in the future for people interested in the Chesapeake Bay problems to work together on the subject, irrespective of state lines.

Naturally close contacts with the work of the Federal Government were necessary. What the United States Government had already built for its own purposes had a bearing upon what the states or municipalities should do or could do for themselves.

Of course, matters of fisheries and wild game life are becoming more and more tied-in with the Federal Government. The Chesapeake Bay should be more productive of fish, oysters, wild fowl, etc., than ever before. The sanitary conditions of the Chesapeake Bay Country, which on the whole are quite good, can be improved readily. We were able to be of some help in arranging for new water systems, sewage disposal plants, etc. The surveys, which we were helpful in securing, such as that of the Hampton Roads District, will be of great importance.

Naturally we ran into clashes, real or apparent. For instance, how far did the stimulation of industrial life interfere with the promotion of recreation and general living facilities? In other words, factories, canning establishments, etc., add often to economic activities and wealth of a community. Would such industries detract somewhat from public health and the joy of living? If so, how could such ideas apparently

antagonistic be made to work together satisfactorily? The discharge of bilge oil in the Chesapeake Bay, the deposits of phosphorus from the Aberdeen Proving Grounds were injurious to the wild fowl life. These matters received our close attention. Some clashes quite unexpected to some of us arose. For instance, health authorities believe that draining certain swamps would lessen the number of mosquitoes. These swamps, however, have their value as feeding grounds for certain forms of wild life. Which idea should prevail? The question is still somewhat of a knotty one, but a solution should be found.

Studies of the mountain regions of Kentucky, Tennessee, Virginia, West Virginia, Maryland and North Carolina had much in common. Shifting trends were interesting as to dominant industries. For instance, the Eastern Shore has known wide variations as to the relative importance of tobacco, grain, sea food, fruit, trucking and canning.

Often my regional office, because of its daily contacts in Washington, was able, by working in close coöperation with the state advisory boards, to assist materially in carrying out in a practical way improvements which had been planned in advance. An illustration of that was the arrangement by which the Federal Government and the State of Maryland at a cost of approximately three quarters of a million dollars are dredging a channel at Ocean City, Maryland, and building retaining walls. The purpose is to connect the Sinepuxent Bay, a large body of fresh water, with the Atlantic Ocean. The result will be that the Sinepuxent Bay will be splendidly adapted for the raising of oysters and certain other forms of salt water seafood, besides furnishing a good harbor from the ocean.

In a rambling fashion, I have touched upon several general aspects of our regional work and have covered a bit specifically two or three of the general planning activities with which our board was concerned. In some respects I should say that they were matters of survey and a study of existing conditions rather than of definite planning. Stress was laid by us upon

planning for road building and bridges at this time or in the near future. Sanitary and health projects have received close attention from us. I think that we know a little more, at least, than we did before what is desirable in regard to such matters. We see something more of a goal. We have outlines of a general plan, somewhat shadowy, it is true, but still something of a plan.

In the main, I feel that the most helpful things which we have attempted to start have been matters which might be classed rather roughly as economic. To illustrate, eradication of slums, improved methods of arterial highways in and through cities have values, economic, of course, as well as scenic and aesthetic. Various aspects of ways and means of subsistence have necessarily come into our studies. The severe financial problems of so many of the people and the extraordinary efforts, which the Federal Government has made through the N.R.A., P.W.A., C.W.A., Emergency Relief, various agricultural boards and numerous other agencies gave increased opportunities for study. Working with these various federal boards as well as numerous state ones, we have been attempting to get a somewhat better picture of the economic conditions in our region with two special ideas in mind. One is that something could be done in the emergency program to remedy the situation. The second is that we could get together some rudiments on which to base planning in rural and insolvent communities.

Naturally in our studies limitation of state lines had to be disregarded very largely. What would be the advantage of studying the coal situation, for instance in Maryland, without attempting to analyze that of the coal region, a part of which only is in Maryland, and of certain other coal regions, especially those having the same markets and where similar competitive conditions as to labor and other industrial factors exist? Why study the coal situation without attempting to analyze that also of competitive fuels, such as oil? Questions of labor, critical industries, competitive and non-competitive industries, transportation facilities and rates, costs of living

and countless other points were involved. What are the basic industries in certain localities? What has been their experience? What guidance of them, if any, should be attempted by Government and how far should private economic factors be allowed to control the situation? I cannot do more here than to refer merely to a few of the topics.

In conclusion, I would like to state that probably never before has there been such an opportunity to make economic studies of this country by governmental agencies as exist today. How far the government should participate in attempting to regulate these industries may be a moot question under normal conditions. How far the financial distress still existing in this country with the millions of unemployed justifies expenditures by the Federal Government avowedly for the purposes mainly of relieving such distress may give rise to differences of opinion. We all know that we have been facing economic problems so unusual and serious that some solution must be found even if unusual in type.

When the present problems of the so-called New Deal have become a matter of history, it is fair to assume that the gigantic and comprehensive efforts made by governmental agencies to handle the distressed conditions of the present era will doubtless be a subject of close study. Aside from the physical things done, I believe that one of the real advantages which will result from these planning and remedial activities will be a far greater increase in the focusing of attention on many economic and industrial problems, than has ever occurred before in the history of our country. The attempts on a far-reaching basis to study these and to set-up some plans for their solution are far reaching. These plans may, of course, be followed, modified or ignored very largely. Possibly some combination of these results will develop quite different from what any of us imagine. Such surveys and such plans should be and will be of inestimable value to the thoughtful minds of the future.

ÆSTHETIC AND SOCIOLOGICAL ASPECTS OF CITY AND REGIONAL PLANNING

JACQUES GRÈBER

(Read April 20, 1934)

THE notion of *Planning* in its comprehensive present meaning attains the highest and the broadest conception of æsthetics and sociology. Based upon realistic facts, built of technical applications, justified only if its scope can be reasonably and financially executed, planning then carries with its physical achievements, indirect and unlimited advantages, beneficial to the moral, social, and intellectual betterment of mankind.

It is a great mistake to consider regional or city planning just as a special *branch* of *engineering* or *architecture*. Seen from this narrow angle, the work of the many technicians who must coöperate to analyze the needs and find out the solution of a planning problem is unjustly underestimated.

To make clear this important remark, we shall simply compare various specialties involved in a city plan: the sanitation and the road engineers, for instance, have a definite task, essential in the final success, but purely technical, while the housing or the landscape experts, beside their knowledge of construction, grading or botany, must have a sure judgment of human aspirations, and tendencies, in order to impregnate the first conception of their architectural or park studies with an imaginative and pleasing charm, as the principal condition of success.

Nobody would deny the important influence of the aspect of a residential development or of a recreational center, on the life of the people for which they have been established.

In other words, the plans of the region, setting and frame of several urban agglomerations, are respectively comparable

to the plan of each of the many houses of which they are formed.

Technical qualities alone make a house *strong* and *healthful*, but are not sufficient to make it *convenient*, *agreeable*, and *beautiful*. *Creation* is not only the result of material researches, calculations, and scientific rules, but needs personality, psychology, invention, and imponderable gifts. In one word, Burnham, that great planner and architect, had the creative genius necessary for house and city planning.

Another proof of the far-reaching aspects of comprehensive planning is in the harmony in which the number of different men, who must contribute to its success, work unselfishly and joyfully, just because they serve a *superior aim*.

The first part of their work, the civic development survey, beside geography, geology, topography, hydrography, climatology, demography and engineering, in many ways involves the help of history and sociology. And their second task, the synthetic elaboration of the plan, calls for the joint and close collaboration of the architect and of the engineer with the jurist, the hygienist, the administrators of public affairs and of finance. Otherwise the plan, even if technically perfect, might remain a beautiful but still-born design.

Planning, shall we conclude, from such a long list, almost discouraging by the number of scientists and specialists involved, is not a *one man's* work. Who, then, is the planner?

The planner will be the coördinator of the efforts of all his associates, also their leader and their *conductor*.

In medieval times, the building trades were also conducted by the "maître d'œuvre," and the cathedrals have stood, through the centuries, as a proof that human coöperation is not a myth.

We are far from city plans and regional maps, but the more we try to investigate how to make them realistic and useful, the more we shall have to admit that spiritual forces must be applied to the conception of them.

Let us take the first elements of the plan: *Circulation* and *transportation*, which bring life into all parts of the urban

or regional body just as does the circulatory system of the human body. How can we harmonize the various elements of the normal layout, without duplication of roadways, railways, waterways and now airways, unless a full and unselfish agreement of opposed and competitive interests be reached?

If we consider now the occupied territory, in its present amazing disorder, and if we try to assign a normal place to the different activities, *commerce*, *production*, *recreation*, and the most important and vital, *housing*, what can we hope to achieve, in this perfectly reasonable effort to plan the whole, if we do not proceed exactly as we usually do when we plan any of the details of it, e.g., houses, farms, industrial plants, commercial buildings, etc.?

After the general survey of the disorder, a careful long range program for a gradual return to order may be planned.

A striking example of the physical need of a regional plan is given by Paris. In 1919, a law was voted prescribing that every French town of a certain importance should prepare its municipal plan and it was only thirteen years later, in 1932, that the need of a comprehensive plan of the entire region was realized. The law for the comprehensive plan was finally voted, but what a waste of energy, money, and time. A large number of communities, included in the region, within a circle of a 20 miles radius from Notre-Dame de Paris, had already completed their local plan, in absolute ignorance of the neighbouring towns, so the work of the regional planning committee is complicated by the difficulty of coördinating (and often correcting) separate units, into one comprehensive system.

And the last but equally important feature of the city or regional plan, *beauty*, either natural or architectural, has to be considered, at least from the economic standpoint. It is a common-place to say that *beauty pays*, but it is so true, however, that many great modern cities are now suffering *financially* for having neglected, during their recent uncontrolled growth, to create and to preserve æsthetic assets, to which other more lucky towns owe to a great extent their

prosperity. Haussmann gave, under Napoleon III, the western sections of Paris an amazing real estate improvement. In 60 years, properties worth ten cents a square yard reached the value of one hundred dollars, a thousand-fold increase. This principle has been applied quite recently to the creation of Fairmount Parkway, the green wedge of Fairmount Park in the heart of Philadelphia. And we do hope that, in spite of temporary hard times, the opportunity, perhaps unique in the world, of carrying out the completion of its original layout will not be spoiled. The wide development of open and green spaces for the material happiness of the congested agglomerations has shown its effects in most of the German cities, where an energetic policy of the "green" has been maintained (the "Grünplan" of Leipzig is a symbol), and we must not neglect this example, especially in the "olde greene towne" dreamed of by William Penn.

We find the best encouragement to express this hope, when we see the admirable work achieved by the Regional Planning Federation of the Philadelphia Tri-State district, which, on a territory forty-five hundred square miles in area, with three and a half millions of inhabitants, has shown the way to order, to better circulation, to more facilities for every kind of human activity, in a common setting of natural beauty and easier life. The same great example was given by the city of Washington, where the original L'Enfant Plan is being emphasized and amplified, on a scale at present of great magnitude in the National Capital, and also by the New York Regional Chart of which certain parts are already completed, such as the layout of Westchester County—vivid lessons to the city planners of the whole world.

The old cities of Europe give other interesting encouraging examples. Marseilles, 25 centuries old, and now the first Port of the Mediterranean and the second city of France, has unknown natural resources in its enormous municipal territory of 60,000 acres. The city plan was then comparable to a regional plan, since the normal need for city extension, estimated for a period of fifty years, would not require the

planning of more than the half of the territory. The outer half, consequently, has been planned as a rural and touristic region, with very strong restrictions on land equipment and building. The southern part of this voluntarily unequipped section offers the most beautiful possibilities for natural park reservations. In such a case, proper planning means just *protection*, and this shows where the planner must carefully avoid his artificial interference. Wilmington gives another splendid example of natural beauty (Brandywine Creek) where the absence of the planner is a great tribute to his taste.

Protection of nature leads us to speak of the protection of historical, archæological beauties, and we must point out what it means; not only beautiful buildings as witnesses of a great period of the past should be protected, but also groups of buildings, which very often without particular architectural qualities, if taken separately, form a picture, by their general proportions or character, worthy of our care, maintenance and protection against modern spoiling.

That has been the case for the whole "Vieux Port" in Marseilles, notwithstanding the reconstruction of many crumbling, unhealthy houses where some picturesque charm has to be sacrificed to safety. But in such cases, the study of the new street layout, together with the building regulations, will keep skylines, proportion, character, roof angles, similar in the modern houses to what they were in the old, with just the addition of air, sunlight and cleanliness. In every part of his work, the planner finds similar problems as we have tried to explain. The accuracy of his researches, the skill of his technique, the methodical organization of his coöperation with his associates, will be assured of complete success if he keeps in mind that the whole of his work is *entirely subordinated* to its social and moral influence on the people for whose life, interests, and happiness it has been intended.

THE ECONOMIC IMPLICATIONS OF NATIONAL PLANNING

FREDERIC A. DELANO

(Read April 20, 1934)

THE expression "planning for the future" has come into very common use in recent years, and yet it is evident from what is said upon this topic that there are widely different points of view. Obviously planning must be for the future, for we cannot change the past, and we know that correcting mistakes is costly business.

In the broadest sense planning is *organized foresight*. It is a deliberate, conscious effort to foresee the trends of development and plan to meet them. A badly conceived plan may do much harm and prove a costly straitjacket, whereas a less definite, and therefore more flexible plan, will produce better results. The plan of a City, State or Nation is not like the plan of a house, a completed project; it is rather a living, growing and developing scheme of adapting one's environment to future requirements.

National Planning deals first of all with physical and basic facts. For example, the population which a country can support depends upon its soil conditions, its natural resources, its climate and rainfall, and its contacts with the outside world, as well as the intelligence and industry of the people. Its great cities on the seaboard will grow up only at advantageous seaports, and those in the interior must have lakes or rivers affording strategic locations, ample water supply and transportation facilities. The chief arteries of traffic, whether by sea or land, must necessarily depend upon the physical geography of the country, the mountain passes, the nature of the terrain, as well as the productivity of the soil.

On the other hand, a National Plan may concern itself more with economic or administrative factors than with physical factors. Thus the Russian Five Year Plan relates

itself in part to a system of governmental leadership and control, as well as to an economic scheme of social welfare and economic rehabilitation. The same may be said of other far reaching and interesting plans of other European nations.

In America there has been some confusion of thought due to the wide interest in City and Regional Planning of urbanized areas with which many of you have been interested in the last quarter century. For this reason it has been too often assumed that National Planning is related solely to the physical problems of means of communication, land uses, development of community centers, etc. The fact that those interested in the physical aspects of these problems have in most cases fully realized the economic and administrative implications of their recommendations, has not been sufficiently realized nor perhaps emphasized. It is notable that the political and administrative direction of our great cities, as well as of our several States, or of our Nation, has too often ignored the very important economic implications of physical developments. In a City Plan, while we may realize that the location of its main arteries of communication plays an important part in its economic as well as its physical development, we may not appreciate that the location of its schools and playgrounds may have an important effect in stabilizing values by reason of creating community or home-owning centers which will endure. On the other hand, a great city may build subways at great expense, on the credit of the entire citizenry, and in doing so not realize the important economic effects which may or may not prove beneficial. Some of its citizens may realize much benefit at the expense of others. Such a scheme of development may blight large areas, or may enrich certain areas beyond the zone from which it receives taxes. So, too, when a city builds bridges and tunnels to reach neighboring areas, does it estimate that it may cause a deflation of its own population?

When, therefore, we talk of National Planning, we should have in mind not only the planning and constructing of important public works, but we must also realize the significance

of the economic, social, administrative or governmental effects which may flow from our proposed actions.

To illustrate what I have in mind, it is peculiarly fitting that in this historic city of Philadelphia we should recall some of those important steps in our National Plan for setting up our scheme of Government. The adoption of the Constitution was a fine example of an effort to plan for the future, and as we read the debates of the period we can not fail to be impressed with the extent to which our forebears, conscious of the old world ills, sought by constitutional limitations to protect the liberty and the rights of the individual. Through an elaborate three-headed Government, and a scheme of "checks and balances," they sought to make dictatorship impossible, and yet to build a plan so flexible that in great crises the Congress of the people would have practically unlimited powers.

Experience has shown that these checks have tended to make Government cumbersome and expensive, but for all that most good citizens are sincere in an effort to cure these ills within the framework of the Constitution. Have we not found not only how to amend the fundamental law, but how to beat a retreat and undo an amendment which we have tried and found wanting?

Alexander Hamilton, the first Secretary of the Treasury, in the administration of George Washington, introduced his plan for the economic salvation of the Nation, which he called the *American Plan*. That was a notable project for industrial development. That plan was adopted even though Thomas Jefferson, his colleague in the Cabinet, differed very sharply as to its wisdom. Hamilton, as you know, believed in a strong central Government, whereas Jefferson believed in strongly entrenched States, held together in a rather flexible federation. It is evident to us now that by the slow process of trial and error, we have come more and more to adopt the Hamiltonian doctrine. Notwithstanding that, however, Jefferson lived to see many of his plans mature into reality.

Jefferson was largely responsible for the George Rogers

Clark expedition which added the vast territory north of the Ohio and west of the Appalachian Mountains, and saved the valley of the Mississippi to the Nation. It was he who put through the Louisiana Purchase and sent out the Lewis and Clark expedition to report on the nature of the area thus acquired. It was he who introduced the township system of surveys of public land, a system well adapted to prairie lands, but unsatisfactory when applied to a highly accidented terrain, or to a land cut by streams. An important economic result of this method of land survey was that it forced the highways to conform largely to section lines, east and west, or north and south—sometimes rather inconvenient. An interesting feature of this township survey was the allocation of two-mile-square sections in every township of thirty-six sections, as school sections, thus resulting in an early determination to dedicate one-eighteenth, or say $5\frac{1}{2}$ per cent of our national wealth, to the maintenance of public school education.

Our National Banking system has been an important feature of National Planning. Two attempts were made to establish a Central National Bank, followed by a State system, and finally by a wide system of State and National Banks, which in recent years have been coördinated into our present Federal Reserve System.

Our National Tariff Policy was primarily an effort to build up home industries so that we should be independent of foreign nations. It was a scheme which looked to our economic independence and security, but not intended by the founders to go to the limit of isolation. On the other hand, as our country has grown in population, in wealth and in importance, it has proved that the provision of the Constitution forbidding tariff barriers between States has developed a competition of free trade between States, unheard of in foreign countries.

Our land tax system, in effect a capital tax on land or its value for a prospective use, was important as a spur to a forced draft method of development. So long as it was

coupled with an open-door policy of immigration it worked well; but in the last ten years it has raised a question of its propriety. People were willing to pay a capital tax on land far beyond any income justification, so long as land prices were being marked up by reason of the steadily increasing values due to a rapidly increasing population; but now that our policy has been changed in respect to immigration, there is a marked discontent with our land tax system.

You may say that the examples I have cited stress chiefly economic measures and have no close relation to great public works, so that it is in order to point out how public works may also have important economic implications.

Take, for example, the great efforts of the early pioneers to build traffic highways across the Alleghenies—and notable among these the Erie Canal in the Mohawk Valley of New York, connecting the tidewater of the Hudson with the Great Lakes. Simultaneous with its completion came the invention of the "iron horse," and then followed the great railway developments, without which our central States would have been only sparsely settled. The hundred years of railway building, encouraged by grants of land, and in some cases, of money, from the States and the Federal Government, have resulted in building up a railway system of which Americans have reason to be proud. Many of our great cities owe their existence almost entirely to this means of communication, and yet as we view the past from the vantage ground of experience, we can now realize that a great deal of this railway mileage, built during a period of unregulated competition, was both unnecessary and unwise and therefore wasteful. Even though it is estimated that less than five per cent of the capital cost of the railways can be attributed to the value of gifts of land and money by the States and the Federal Government (an amount less than is paid by the railways in taxes each year), still it is apparent that much of this useless mileage must, as the years go by, be amortized and torn up, or better still, converted into public highways for a more general use. The burden of thus amortizing this

useless capital becomes a general burden upon all the people, no matter how it is dealt with, whether it falls upon the creditors of the transportation companies, or out of higher transportation charges, or by general taxation.

In more recent years, the Panama Canal is a noble example of a great public works undertaking which has had profound and unexpected National and world wide effects.

Other examples are the great reclamation projects of the western country which have had far reaching effects, as no one can doubt; and it is generally true that the water supply and power systems of our chief metropolitan areas have had vast social effects. The experiment being made in the Tennessee Valley is not only the most recent, but also the most interesting, deliberate effort at developing an area notable for its great resources, yet suffering from isolation. This area of 45,000 square miles, representing an entire river drainage system, includes most of the State of Tennessee and a part of six other States. The effort in this case is to make the highest possible economic use of the land, and at the same time build up the population by giving forethought to the social and economic well being of the inhabitants. Plans for other large areas transcending State lines are also being considered and may be dealt with if they give promise of success. Among these I may mention the New England group of States, the Columbia River Basin, and the Colorado Canyon States.

I have doubtless said enough to appeal to your imagination. You can see that *Planning*, in a sense, is *a state of mind*. It is, or should be, based on education in which all true citizens should, and indeed must, take an important interest. It is founded on the theory that every well wisher of his country, every man loyal to the traditions of the founders, every man who wishes to apply to the welfare of his country the foresight and planning which he now devotes to his private affairs, should want to have a part in it.

From another point of view, physical planning is an attempt to put in as definite terms as possible a forecast of

future requirements. Naturally the physical side of many of these problems appeals most to our imagination, for it is the character of the country in which we live that determines to a great extent how we shall live and what we shall do. In what I have already said I have stressed sufficiently the fact that in carrying out plans for our physical well being, or for the development of the country, we are bound to produce important economic, social and governmental results.

When our forefathers had a vast empire in which to spread out, the greatest problem before them was cutting down the trees so as to build their homesteads and lay out their garden tracts. Protection against hostile Indians and against the severities of the climate was in itself a very big job, but it was largely the work of individuals in which group effort and community coöperation were not in the forefront. There was a great deal that was *fine, noble* and *inspiring* in the rugged individualism of that time, but we have outgrown those conditions. Our pioneer days are over. We are living in great communities where, whether we like it or not, we must give heed to the desires and necessities of our neighbors. No honest and thoughtful man who has considered what has happened in the last five years can fail to appreciate the importance of planning. It matters very little whether we stress as most important the physical side of the problem, or the social, or the economic. These can not be disassociated one from the other. Obviously they are all closely related and intertwined.

The principal job before the National Planning Board is to get across the idea of the importance of giving thought for the morrow. A drifting policy will not do. What is sometimes called *laissez faire* might better be called "*lazy*" *faire*, and only lazy-minded people will accept it. Fundamentally, too, National Planning is largely a matter of coöperation and coördination. Every Government official from high to low, if he is worth his salt, is planning how his department or bureau shall be developed. That is proper enough, but we must persuade these men and women that while we find no fault

with them, we do insist that these plans shall be brought out into the open and coördinated and built into a completed fabric. In planning a city, who would be satisfied if the school board, the library board, the playground commission, the highway engineer, should each make their plans regardless of the plans of the others? It is too obvious to need argument that these plans must be brought together and coördinated, and the same is true in larger scope of State and National planning. Even in State planning it has become increasingly evident that one State can not do successful work without coöperating with its neighbors. I recently attended a conference at which it was brought out that one State was spending millions to prevent the pollution of a stream common to three other States, none of which was doing anything in this direction. So I may say as my closing word that the biggest job of the National Planning Board is to convince the people from old to young and from young to old that a broad scheme of National Planning, suitably oriented and coördinated with an intelligent basis of priorities, is not only an essential necessity, but will prove of great value to those who come after us. Furthermore, there must be a continuity in this effort through boards composed in part of ex-officio departmental chiefs and in part by private citizens.

STABILIZATION OF THE CURRENCY

RAY B. WESTERFIELD

(Read April 21, 1934)

I. DEFINITION

WHILE the value of the dollar has always varied, in both the home and the foreign market, in the recent period it has been subject to unusually wide, sudden, capricious and unpredictable changes. What might be regarded as the "normal" variation, that growing out of the circumstance of agriculture, industry, and domestic and foreign commerce, has been upset by the unprecedented conditions wrought by the War and by the current depression, so that the seasonal, cyclical and secular tendencies computed and used by our forecasters no longer hold; and, in addition, the Administration has multiplied the frequency, range and irregularity of fluctuations by its capricious policies as regards money, foreign trade, and finance. The revolutionary character of the "New Deal" and the very considerable shift of so much of government from legislative to administrative control have accentuated the uneasiness born of the breakdown, real or threatened, of the hallowed institutions that made for stability in times past, such as the gold standard, the banks, agriculture, and even capitalism itself. The crying need of this bewildered floundering and unsteady world is stabilization, not alone of the currency, but of almost every other human institution.

The stabilization of our currency refers, on the one hand, to the stabilization of the purchasing power or value of the dollar in our domestic markets—the stabilization of domestic prices—and on the other, to the stabilization of the purchasing power of the dollar in the foreign money markets—the stabilization of foreign exchange rates. The internal and external values of the currency are, of course, related, tending

toward equality as far as permitted by tariffs, transportation costs and other obstructions. There are temporary disparities, because the value of the dollar is affected by different forces in the home and foreign markets, but given an opportunity through international commerce, these differences of purchasing power will be erased by changes in relative price levels here and abroad. If our domestic price level is stabilized, the adjustment must be made in the foreign exchange rate or purchasing power abroad. Those who "manage" the currency so as to maintain a stable domestic price level must perforce become relatively indifferent to fluctuations in the foreign exchange rates and the handicap they put thereby on foreign trade. A rounded treatment of the subject of stabilization of the currency would cover both the internal and external values, but except indirectly, my discussion will be limited to the stabilization of domestic prices.

II. THE ROOSEVELT STABILIZATION POLICY

President Roosevelt has stated his views on money and prices on four occasions. Over the radio on May 7 he said that his administration had "the definite objective of raising commodity prices to such an extent that those who have borrowed money will, on the average, be able to repay that money in the same kind of dollar which they borrowed," but that he did not wish to give borrowers "such a cheap dollar that, in effect, they will be able to pay back a great deal less than they borrowed." On July 3 in his message to the World Economic Conference in London he reaffirmed his belief that our national currency should have "a continuing purchasing power which does not vary greatly in terms of the commodities and need of modern civilization," and added: "Let me be frank in saying that the United States seeks the kind of dollar which a generation hence will have the same purchasing and debt-paying power as the dollar value we hope to attain in the near future." In his radio address on October 22 he announced his plan to buy gold with the object of depreciating the value of the dollar and thereby

raising the prices of commodities to a level which would, among other things, "make possible the payment of public and private debts more nearly at the price level at which they were incurred." He said further: "When we have restored the price level, we shall seek to establish and maintain a dollar which will not change its purchasing and debt-paying power during the succeeding generations. . . . Our dollar is now altogether too greatly influenced by the accidents of international trade, by the internal policies of other nations and by political disturbances in other continents. Therefore, the United States must take firmly in its own hands the control of the gold value of our dollar. This is necessary in order to prevent dollar disturbances swinging us away from our ultimate goal, namely, the continued recovery of our commodity prices. As a further effective means to this end, I am going to establish a government market for gold in the United States. . . . This is a policy and not an expedient. It is not to be used merely to offset a temporary fall in prices. We are thus continuing to move toward a managed currency."

On January 15, the President submitted to Congress a bill which, with some minor amendments, became on January 29 the so-called "Gold Reserve Act"; the message which accompanied this bill said that "In conformity with the progress we are making in restoring a fairer price level and with our purpose of arriving at a less variable purchasing power for the dollar, I ask the Congress for certain additional legislation to improve our financial and monetary system." He asked that the gold of the Federal Reserve be appropriated; that the upper limit to which the dollar may be devaluated be set at 60 per cent (the lower had been set by the Thomas amendment at 50 per cent); that in order "that we may be further prepared to bring some greater degree of stability to foreign exchange rates in the interests of our people, there should be added to the present power of the Secretary of the Treasury to buy and sell gold at home and abroad, express power to deal in foreign exchange as such," and that "as part of this power . . . out of the profits of any devaluation,

there should be set up a fund of two billion dollars for such purchases and sales of gold, foreign exchange and government securities as the regulation of the currency, the maintenance of the credit of the government and the general welfare of the United States may require."

The gold certificates given by the Treasury to the Federal Reserve banks are payable in dollars of such gold content as the President may from time to time determine, between 50 per cent and 60 per cent of 23.22 grains. On January 30, the President set it at 59.06 per cent for the time being, but expressly reserved the privilege of changing the figure if conditions require it. This is tentative or temporary devaluation but not yet stabilization of the dollar.

The President gave as his official reasons for the act of devaluation the facts that he had concluded it was necessary to protect our foreign trade from the effects of depreciated currencies in competing nations, that it was desirable to stabilize domestic prices, and that domestic conditions call for an expansion of credit. These are the bases on which he may, from time to time, alter the gold content of the dollar. Nothing was specifically said about stabilizing the price level by varying the metallic content. There was no commitment to the commodity dollar in the specific form espoused by Professors Fisher and Warren. Nor is there any commitment specifically to the price level of 1926 or any other year as the proper basis for prices at which level stabilization will be attempted. One can only arrive at an answer to these points by inference from what the President and his advisers have said and from the trend of action.

By such inference it would seem that our national monetary policy is: (1) to raise the price level to the 1926 level; (2) then to stabilize prices at that level; (3) to employ the principle of the commodity dollar; (4) to establish our currency on a fluctuating base of gold, or on a fluctuating symmetrical unit of gold and silver, or possibly on a bimetallic base; (5) to have a "managed currency"; (6) to employ a large fund in the manipulation of the foreign exchanges; (7)

to employ and coördinate a number of nonmonetary factors as auxiliaries in raising the price level and not to depend on monetary means alone; (8) to centralize monetary authority in the Treasury and to make the central bank responsible to the Treasury.

III. OBJECTIVES

1. The first task in executing a system of price stabilization is to choose the level at which prices will be stabilized. Since the Administration has apparently chosen the 1926 level, the reflation of prices to that level must be regarded as a part of the stabilization process. In addition to the promotion of recovery through such reflation, the two major objectives of the stabilization policy, as expressed from time to time, are, first, the attainment of greater equity in the debtor-creditor relationship. This purpose is preëminent. The advocates of stabilization stress the burden to debtors added by a fall in the price level, and the similar exploitation of creditors when the price level rises.

The question of injustice as between debtor and creditor arises mainly in case of long-term contracts for the payment of rent or interest and principal; as for short-term contracts, no serious inequity can be done the parties by price change within the debt period, except at a time of extreme monetary instability. For short term debts the parties ought to be able to forecast price trends and make allowance therefor in the interest charged. In the case of taxes, the rate and assessments are made long before the taxes are paid, and if the increased burden caused by declining prices is to be avoided the taxes must be anticipated and prepaid.

A falling commodity price level does not necessarily result in injustice to debtors. It is true that the debt runs in fixed dollar amounts, the repayment of which entails delivering to creditor by debtor a larger command over goods than was borrowed. If the price level has fallen on account of greater technological efficiency and the debtor's costs of production have fallen in proportion to the price level, he is not penalized in repaying a sum of dollars having greater

purchasing power than he borrowed. It is not desirable to stop a fall of prices that comes from increasing efficiency. To let prices fall gradually as productive efficiency increases is necessary if consumers are to have a fair deal. By keeping the purchasing power of money debts constant the benefits of economic progress accrue to the debtor class. In case the commodity price level is used as the guide for stabilization the level should be corrected for non-monetary influences on price, such as technological progress and the exhaustion of natural resources. A country which stabilized the price level without such correction would on occasion find its export industry and trade injured and its ports flooded with imports, or vice versa.

2. *The avoidance of economic chaos and waste.* A goodly proportion of Warren's "Prices," of Fisher's "The Money Illusion" and other books, which have apparently had great influence on President Roosevelt, is devoted to a description of the misery, conflict, waste and chaos which changing price levels cause. The so-called business cycle is ascribed largely to monetary causes and the evils of secular changes in prices, are stressed. For instance, the relations between wages, rents, profits, interest, and prices are seriously disturbed. Security values are inflated and deflated. The prices of different groups of goods and services are changed in different degrees and some sooner than others, with the result that markets are strangled or boomed. Employment of mills, mines, farms, and carriers is unsteady, entailing poor utilization of the means of production and distribution and much social unrest. The toll of capital and human energy is indeed heavy and constitutes the greatest indictment of our system. Moreover, inflation and deflation become more and more serious as our social organization becomes more capitalistic, with labor more specialized and mechanistic. It is frequently said nowadays that capitalism cannot stand another decade like the last ten years and that the stabilization of prices is necessary to save the country from socialism; that while individuals can safeguard themselves in part

against the harm of unstable money—by forecasting, by diversifying their investments, by basing contracts on a price index—the salvation of the nation lies in a sound and scientific monetary system, one which will provide price stability.

IV. GENERAL CRITICISMS OF STABILIZATION

No one will deny that large secular and cyclical changes in the price level cause much injustice as between the parties to monetary contracts or that the cycles of boom and depression occasion great waste of capital and human energy or that such prostrations as the current depression shake public faith in capitalism. There is no one who does not wish that these defects of our system were corrected or lessened. I wish I were equally confident that the stabilization of the price level would accomplish these ends, and do it without entailing other undesirable results, and that any of the proposed schemes of stabilization were practicable and dependable. Frankly I am not sanguine of possibilities.

I. In the first place, it does not seem to me that price stabilization is an adequate remedy for the cyclical and secular movements in production and distribution. The theorists who advocate stabilization have oversimplified the causation and overemphasized the function of money as a standard of deferred payments. The mere upward or downward movement of a price index is not of itself sufficient to establish a case for monetary correctives. The explanation of the business cycle in purely monetary terms fails egregiously; whereas before the War the quantity theory of money was used quite exclusively to resolve the cycle problem, in the writings of cycle students since the War, with a few notable exceptions, the general price level has ceased to play an important role. "Not the movement of the general price level, but the chronological succession of special prices and price combinations, as well as other time series, are regarded as significant; . . . by a system of curves which culminate at different moments, and by their interrelation, are the ups and downs of business adequately described." (Haberler, Q. J. E.

42 : 446.) The march of events has driven home the highly artificial character of the assumptions underlying the quantity theory, as well as the strong propensity of the theorists to forget that their hypotheses were unreal. Some of the assumptions of Fisher's equation of exchange have been subjected to quantitative statistical measurement and found far from fact. The case for resting monetary and credit policy solely on a single price index falls when the cyclical and secular changes are studied in fullness and with exactness.

2. Not only is price stabilization not a panacea or solvent of all our economic problems, as the stabilizationists would have us believe, but it might have certain undesirable results if achieved. The claimed beneficial effect on industry and commerce is at least contestable in view of the fact that stability has never been experienced in the past and theoretical objections may be espied. For instance, price fluctuations, although sometimes causing apparently excessive profits or losses, may be vital as a stimulus to business activity. Complete stability might neutralize the effects of our competitive mechanism in regulating supply and demand and adjusting production and consumption. The costs of adjustment in a progressive society must be borne by someone, either society as a whole or specified classes in the community; price stabilization protects the fixed-income classes and shifts the payment of such losses to others. Booms can develop quite as well on a stable price level as on a rising price level; indeed, our current depression followed the inordinate boom of 1925 to 1929 when prices were slightly falling; technological progress may offset the contraction of prices and sales at home and abroad on credit provide a market, and together let the boom proceed till it breaks; this was the story of the years 1926 to 1929.

3. In the third place, the execution of any scheme of stabilization is bound to be one-sided. It is now generally conceded that, by determined, consistent and prompt effort the monetary authorities can fairly successfully stem a boom, but that exclusive reliance on monetary measures will not

avail to lift a country out of depression. The confidence of the quantity theorists has been rudely shocked since 1930 in the failure of business to respond to easy credit. Four years of effort along this line during the depression have failed to restore prices and prosperity. It seems now that the best way to achieve price stability is to restrain the booms which cause the succeeding depression. Unfortunately, however, this is the very thing that is not likely to be done. No monetary authorities ever have been or will be courageous enough to check a boom and do it at the proper time; the traditional psychology of our people favors rising prices and prosperity; no administrative authorities could withstand the popular opposition to action openly designed to reverse an upward trend; politicians would eliminate a stubborn administration and even sacrifice the stabilization machinery, rather than allow such a program.

And there are theoretical considerations which justify such a one-sided policy. It is more defensible to check a fall in the price level than to check a rise. The number of years of prosperity per year of depression is consistently larger, both in the United States and England, during periods when the secular trend of prices is upward rather than downward; and if the monetary authorities check the fall in prices they may increase this ratio still more. Rising prices bring advantage to entrepreneurs; they are stimulated to produce and supply employment; and as for the employee, to be employed, even at a slowly rising cost of living, is better than to be idle and on the dole. "So far as can be judged by history, the best periods for humanity have been periods when commodity prices were rising slightly. Any effort to stabilize prices should make sure that no decline will occur, even at the risk of having a slight rise" (Warren and Pearson, *Prices*, 158-9). Only if the decline in price is due to a per capita increase in productivity through improvements in organization and technique is it undesirable to check the fall.

4. Another objection to attempting price stabilization is the want of satisfactory guides to follow. It is difficult to

construct an index number which would operate fairly between all sections of our country, and if the stabilization scheme is made worldwide, it becomes still more difficult to include most of the principal countries of the world and give them proper respective weights. The selection of prices is most puzzling: the movement of wholesale prices does not synchronize with retail, nor do the prices of producers' goods move with those of consumers' goods, and none of them with rents, wages, dividends, and interest, which are forms of price, or with the market values of stocks, bonds, real estate, public utility services, and the like. Quite obviously a wholesale commodity price index does not adequately represent the price situation, and if it be used by the stabilization authorities as the guide to their program there is no assurance that equity as between debtor and creditor will be promoted or that a boom or depression will be checked as desired. It is not at all impossible, for instance, that an increase in the quantity of money be accompanied, not by a rise in the wholesale commodity price level as desired, but by a fall of that index, the money being diverted, along with a lot more, into the security or real estate market. Such indeed was the experience from 1922 to 1929: the credit inflation did not raise commodity prices in that period; it merely offset the downward pressure wrought by increasing technological efficiency and some found outlet in securities and real estate booms.

5. This point introduces another criticism of most stabilization plans. It is a common fault that they fail to face the fact that measures taken to inflate or deflate the price level react differently on different price groups; some being much more sensitive than others, respond sooner and more. Moreover, the character and force of the response varies materially from time to time. "There is no standard behavior pattern in the reaction of prices to monetary forces, no uniformity of response which permits us to specify with any accuracy how the price system will react to stated monetary changes. . . . The incidence, upon the price system, of monetary expansion or contraction is, in the main, beyond the control of monetary and banking authorities." (F. C. Mills, *Pol. Sci. Pro.*, 1934.)

6. Maybe sufficient reasons have been given why I believe stabilization plans are, in general, impracticable, if not undesirable. One outstanding virtue of the gold standard is that its mechanism is comparatively simple and that it is more automatic and requires less government manipulation than any other system. The proposed schemes of "managed currency" are far more complicated and dependent upon government action. Every circumstance affecting public opinion as to the political intention of the government or as to its ability to make good its promises and to maintain stability will cause the value of money to change. No mere regulation of the quantity of money and credit will avail to prevent violent fluctuations if faith in the administrative authority wobbles. A century's experience has proved that the gold standard at least has the advantage of preventing excessive manipulation, and a whole system of safeguards has been developed to protect it. Departure from the gold standard is rightly viewed therefore with trepidation, in the light of history. The monetary difficulties of the post-war period have been due not so much, if at all, to intrinsic defects in gold as a standard, as to the mistakes made in reestablishing the gold standard after the war and the impossible political burdens put upon it.

7. If the United States adopts a plan of price stabilization, it means further economic and political isolation, further drift toward nationalism and away from international comity. As among countries on the gold standard it is impossible for any one by currency or credit to raise or lower its price level much above or below the level prevailing in the other, for the gold standard will automatically equate the levels by affecting the balances of trade and other payments. But when a country has deserted the gold standard, it is free to inflate or deflate as it pleases. The national policy of price stabilization is incompatible with the maintenance of the gold standard. If the United States sought to hold its price level stable and the rest of the world left their levels drift, she could not at the same time keep her currency freely

interchangeable with gold in the world's market at a fixed ratio, for she would either lose her gold or acquire inordinate quantities from abroad, depending upon what level had been chosen for stabilization and how world gold prices trended. Altering the gold content of the dollar on the basis of domestic prices would involve instability of foreign exchange rates and thus handicap our international trade, and maybe lead to competitive devaluation. The position of Professor Warren is that our foreign trade is an inconsequential item in the world's total and as compared with our domestic trade, and on that ground holds that exchange rates are unimportant as compared with domestic prices.

It is to my mind folly to believe that price stabilization by any one important country in isolation from its neighbors is at all likely in the near future. There is little reason for America to hope many important nations can be persuaded to adopt the "commodity dollar" or any other scheme of "managed currency" which she may evolve. In times past no country has found it advantageous or possible to divorce the movement of her commodity prices from the world movement in prices for any extended period. The world is likely to revert to the gold standard; if it does, stabilization must then mean the stabilization of the world value of gold. Monetary efficiency on a gold standard can be maintained only on an international basis. An international standard is preferable to a series of separate national standards, for the inefficiencies of some of them will handicap the efficient management of the others and the seeking of competitive advantages will drive the world down the ladder of devaluation. The new gold standard should be operated under a truly balanced international system of joint control and regulation, designed to secure stability in the purchasing power of gold.

The nationalism of our present administration has already run to the extreme. Our foreign trade is stifled, and all the industries depending upon foreign markets are contracting. Our monetary policy has seconded and bolstered our tariff

policy in killing our international trade. For us to proceed further in this course of isolation in an attempt, doomed in advance to failure, to stabilize domestic prices, would be the height of folly.

V. MANAGEMENT DEVICES

Management of the currency requires a *theory* of the operation of price-making forces and a *mechanism* whereby such forces may be put to work and controlled. The list of management devices that have been proposed is very long. Some are merely designed to create general conditions favorable to stabilization, such as the establishment of public confidence in the credit of the government and of the banks. Others are "price-fixing" schemes applicable to but few things and under exceptional circumstances. Others are mere reforms of traditional institutions, such as the gold standard and bimetallism, by which it is believed they can be protected against abuse and be made to stabilize prices, others are involved heavily in the recovery program, and are only incidentally to function as stabilizers of prices, such as the public works program and the scheme of equalization payments to farmers.

In the time allotted me I shall discuss but two stabilization devices, namely, *central bank credit control*, and the *commodity dollar*. These two are by far the most frequently mentioned as possible, practicable and desirable methods. They are not exactly competitive substitutes, for the latter would require the former as an auxiliary. The world has had considerable experience with the former, but only meager partial trial of the latter. The former has been advocated by Professors Wicksell, Cassel, Keynes, and Hawtrey, and the latter by Professors Fisher and Warren.

VI. CENTRAL BANK CREDIT CONTROL

I. *Propriety of the function*. The record shows that any responsibility undertaken by the central banks for maintaining stability of value of money will be assumed reluctantly and only under public pressure. The growth in power and

responsibilities of central banks has been slow, unconscious and unplanned, a haphazard evolution, as one necessitous occasion after another thrust this or that duty upon them. After 1914, there was heaped upon them the task of financing the World War, and since that event they have played prominent roles in national and international financing. The resolutions of the Genoa Conference in 1922 were epochal in that for the first time the responsibility for maintaining the value of gold was specifically put upon them. Before the War the gold standard system which was in general use was regarded as almost automatic, requiring no attention from the central bank beyond protecting a gold reserve sufficient to provide convertibility of the national currency. The opinion and practice of the central banks since 1922 have been opposed to the Genoa plan, and little progress has been made in getting them to do more than maintain their gold reserves. They have argued that it was an impossible task, reasoning that price movements were not wholly of a monetary order, but due to industrial disequilibrium and to psychological and foreign factors over which they had no control, and that currency management would provoke international economic conflict. Meantime the violent movements in price levels in the various countries—with inflation, devaluation, restoration of the gold standard, defection from that standard, and the sinking of price levels the world over during the current depression—have raised a public demand that the government or the central bank or both together stabilize prices.

The debate continues among bankers and monetary economists whether central banks can stabilize prices and whether, if they can effect stability, they ought to assume the function. Professor Cassel leads one wing declaring that "the general level of prices is exclusively a monetary question" and that "the Federal Reserve has no other function than to give the country a stable money." He believes that "long practical experience affords decisive proof of the possibility for a central bank to regulate its supply or means of payment

so as to secure a definite purchasing power for its currency." Professor Hawtrey holds that "the new task would not be different in kind from that which they have always undertaken" and "the technical methods of credit regulation would be the same." Professor Keynes, writing in 1932, while acknowledging that he "had more sympathy than he had a few years ago with some of the doubts and hesitations such as were expressed by Governor Strong." Professor Stewart and other witnesses before the Committee on Stabilization, "reasonable doubts expressed by persons of great experience," which "can only be dispelled by the prolonged success of an actual attempt at scientific control," nevertheless regarded "the prospects of such an attempt sufficiently promising for it to be worth a trial." The Labour Party is demanding that the Bank of England use its powers to stabilize prices, under threat to nationalize the Bank if it does not do so. Although this Bank subscribed to the Genoa resolution in 1922 and was assigned the task of calling a conference of the leading nations to study the problem of "preventing undue fluctuations in the purchasing power of gold," it has continuously postponed calling the conference; and although after the Ottawa Conference Chamberlain promised British co-operation in raising the price level of England and the Dominions through monetary means, it was evident at the London Conference that England intended to take no positive action to raise the price level. In fact, Governor Norman does not believe central banks have much power over price levels and is opposed to using the Bank of England for that purpose, and he has withstood the recommendation of the Macmillan Committee in 1931, which declared: "The aim of the Central Banks should be to maintain the stability of international prices both over long periods and over short periods."

A. Barton Hepburn, who had a profound influence on American bankers, pronounced it "one of the most dangerous proposals which has been made in connection with the rediscount policy of the Federal Reserve Banks . . . by

varying the rediscount rate to hold the general average of commodity prices in the United States at a fixed level." B. M. Anderson has assailed commodity price stabilization as "a false goal of central bank policy," resting on a "dangerous economic fallacy," and as "fantastic . . . to expect central bank policy operating *via* the money market . . . to fix the level of commodity prices." Professor H. P. Willis vigorously declares that "the problems of the central banker which have had to do with the control of prices are imaginary and for the present should be laid completely aside until the theoreticians have done. Prices do not afford a field for central bank tinkering, least of all for the tinkering of inexperienced hands like those which control many central institutions."

I can not take the time to recite further views on whether price stabilization is a proper and practicable function for central banks. In general it may be said that it receives practically no support among central or private bankers here or abroad; that abroad it is advocated by Cassel, Hawtrey, Keynes and some other monetary theorists; and that in America it is supported by a few quantity theorists, especially Fisher, Warren, Edie and Rogers, and opposed by practically every student of the Federal Reserve, including Willis, Beckhart, Reed, Spahr, Bradford, Harris, Kemmerer, Steiner, Sprague, and Burgess.

2. *Theory and means of control.* To take a position on the propriety and practicability of this function for a central bank requires the formulation of a theory of the relationship of central bank credit to the price level, as well as of the means of control of that credit and, therefore, of prices. While this is the core of the whole problem, its exposition would have to be too extensive and abstract for presentation here. Moreover, the theorists do not agree in details or emphasis and the delineation of these differences would be painful to this audience. Let me, therefore, state briefly the means proposed.

(a) *Manipulation of the bank rate.* The effect upon credit is variously held to operate as a sieve to thin out the demand

for loans, by affecting the cost of production and distribution, by influencing the flow of savings to investment or consumption, by modifying the international movement of short-term capital and gold.

(b) *Open market operations.* This device can be used in lieu of discount rate change or in support of it. Purchases in the open market promote easy money conditions, sales do the opposite.

(c) *Sterilization of gold.* An influx of gold into the reserves of a central bank provides the basis of an expansion of credit, but the central bank may counteract the expansion by selling an equivalent amount of securities or by paying the gold into circulation instead of bank notes.

(d) *Coöperation among central banks.* Heavy international movements of gold and short-term credit are very disruptive to the monetary stability of both the receiving and sending nations. If the stock of monetary gold reserves is held by the central banks, it would be possible by common agreement and action to minimize the gold movement, as well as to economize it on a gold exchange standard.

(e) *Gold purchase and sale.* The central bank may influence the gold movement by the prices at which it offers to buy and sell gold.

3. *Degree of control possible.* In weighing the degree to which it is possible for central banks, by the means suggested, to control the price level it will be well to consider three types of variation. In the first place it is certain that under no type of monetary system is it possible to resist inflation in time of great wars or deflation in time of catastrophes, such as the present depression. It is essentially unfair to judge either the gold standard or "managed currency" schemes by recent events, for while the world is in such topsy-turvy state as it has been since 1914 it is impossible to operate any system with success. No system could withstand a complete breakdown of a banking system such as befell ours since 1920; an unsound commercial banking system such as ours is a fatal weakness and cannot be the foundation

upon which a successful price stabilization scheme can be built.

In the second place it is doubtful whether central banks can do much to oppose upward or downward secular movements of prices. Professor Warren says that "by management of credit it is possible to throw commodity prices out of tune with gold by a limited amount" but "there is no indication that any permanent change in this relationship can be accomplished in this way." A number of prominent economists after 1925 argued statistically and otherwise that the world was facing a scarcity of gold which would drive the world price level downward indefinitely beginning in the early thirties. The computations and premises upon which this forecast was built were not universally accepted, but the forecast did provoke thought as to what central banks could do to forestall such an unwelcome event. The proposals all aimed at economy of gold and required international agreement (*a*) to withdraw gold from circulation and use it only as reserve; (*b*) to abandon all legislation requiring minimum reserve ratios; (*c*) to abandon consciously all unwritten conventions or traditions by which, in the absence of legal requirements, high reserve ratios are maintained; (*d*) to concentrate the gold reserves in a few only of the central banks,—in those centers which are universally recognized as international money markets; (*e*) to avoid mal-distribution and sterilization of gold; and (*f*) to coöperate in easing and minimizing the international flow of gold. It needs only to be said that instead of proceeding along the six lines of action proposed the central banks followed the very opposite course,—bringing a mad scramble for gold, a dispersion of gold reserves, a worse mal-distribution, and a breakdown of the gold exchange standard. It is an everlasting and universal law that in emergencies selfish nationalism transcends international considerations and that political outweigh economic considerations.

At the present time two of the outstanding forecasters who ten years ago predicted the scarcity of gold are divided

as to the future trend of prices: Warren predicts a downward trend, Edie a pronounced upward trend. The central bank authorities will have a real problem in anticipating a trend which is doubtful in direction!

The Gold Delegation of the League of Nations in 1931 ascribed the mal-distribution of gold to the instability of post-war international finances which, in turn, were due to the *burden of war debts*, which forced Germany to meet her obligations by borrowings from abroad, the *non-productive character* of many post-war loans, the *accumulation of capital in the form of liquid balances* in place of fixed investments, and the *absence of the normal effect of gold movements* on the domestic credit policy of certain countries, notably France and the United States. I cite these facts to illustrate how impossible it is to expect the central banks, dominated as they are with a national interest, to offset these great political and economic forces that were driving prices downward.

Coöperation between central banks in fighting the secular trend of prices is no more likely to succeed than coöperation between the nations in disarmament or protective tariffs. This is especially true when the nations are off the semi-automatic and non-political gold standard and on a system of managed currency standards which are conceived as national instruments defensive and offensive and are destined to competitive depreciation of the currencies.

We conclude that central banks are likely to be ineffective in fighting secular trend of prices. If the country is on the gold standard, gold prices are a world phenomenon, and a single central bank can avail but little at changing their trend. Consistent coöperation among central banks over a long period is quite impossible to attain, at least while nationalism is our watchword and ideal.

The third type of variation of prices which central banks may attempt to smooth is the cyclical. Almost all the stabilization theory has been devoted to this form of price change, because it is a frequent phenomenon, a prominent if not causal feature of the business cycle, and presumably more

easily controlled. The more sanguine, like Professor Fisher, have held that if the price level were stabilized the business cycle would largely disappear.

While a country is on the gold standard its central bank's efforts to stabilize domestic prices are likely to avail little for those prices are dominated by the world situation; the internal situation must trend toward the external, rather than the reverse. If, with a view to lowering prices, the Bank tightens the money market by raising its discount rate or selling securities in the open market, it will attract funds and gold from abroad, which help to sustain the price level; whereas if the country is off the gold standard, any increase of reserves which incoming gold affords is without significance as far as credit or price expansion is concerned. Gold reserve under such circumstances is a myth.

A rise of prices originates without resort to bank credit. The increased demand for goods is effected by means of book credit, bills of exchange, and promissory notes. When the business outlook is promising, the customers of banks lay plans and start operations and by and by resort to the banks for help. It is only after prices have been raised and an upward momentum established "that the wave reaches the central banks of issue. The increase of their circulation does not create an upwards tendency of prices but it is the result of an accomplished fact" (Levinski, 66). "It required, therefore, some time for a rate change to show its effects in the altered lending operations of the banks."

". . . A rise of a discount rate when expectations of the business world are very sanguine and the expansion is in its first stage does not produce any effect at all" (Levinski, 67). If the chances for wide profit margins are great, through favorable position or rising prices, or if costs can be reduced through technique or organization, the increased interest cost may be ignored by borrowers. In many lines the cost of credit is a small part of total costs and a jump in the discount rate from 5 to 6 per cent, although it is a 20 per cent increase, does not materially affect profits.

Although a change in the cost of credit may effectuate a decrease in the volume of bank credit outstanding, it must not be forgotten that the quantity of bank credit outstanding is only one of the many factors that determine the price level. The rate at which money and credit are turning is an equally powerful factor, and it depends on the density of population, the use of checks, bank balance requirements, and the prospect of change of price level. If the central bank tries to offset the velocity factor, it will be as likely to promote instability as stability.

The direct influence of the central bank in the money market is largely confined to the yield on high-grade short-term paper and high-grade seasoned securities, such as bankers' acceptances, treasury notes, and government bonds, that is to items dealt in on the open market. The rates on customers' notes which constitute the bulk of bank loans are slightly affected; they are more customary and fixed, and so changes in the central bank rates are deprived of potency in changing the volume of bank credit outstanding either up or down. Although the demand for short-term funds is highly variable it is inelastic, not responding to rate changes. Raising the rate is more effective in decreasing the volume of bank credit than is lowering the rate effective at increasing the volume. The impotency of easy money policy in stimulating borrowing has been amply demonstrated since 1931. It proved almost equally ineffective in stopping liquidation. Until funds are borrowed and used in purchases of goods the commodity price level will not be raised or recovery started. At the present time the rediscount rate is $2\frac{1}{2}$ per cent, call loan rates 1 per cent and member bank reserves 75 per cent in excess of requirements, and still borrowers have to be importuned to borrow and banks to lend.

The structure of our Federal Reserve does not lend itself to prompt and efficient control. The Banking Act of 1933 strengthened the hand of the Federal Reserve Board over open market operations, over security loans of member banks, and over investment houses. But the Board remains too

large and inexperienced, too prone to debate and delay, too opportunistic and political. Moreover, it executes its decisions awkwardly through the cumbrous machinery of twelve banks. At the present time the Board is completely subjugated to the Treasury, without power to determine upon or execute a policy of stabilization or anything else. Whether it will ever recover its independence is indeed doubtful.

Since 1922 the Federal Reserve has shown no disposition to make price stabilization a major objective. If the price index is used as a guide to credit policy, it must be in conjunction with numerous other factors. The Board holds "that the price situation and the credit situation, while sometimes closely related, are nevertheless not related to one another as simple cause and effect; they are rather both to be regarded as the outcome of common causes that work in the economic and business situation" (Harris I, 88). In 1926 the Reserve authorities denied their ability to control or influence prices, and disapproved the efforts of the stabilizationists to load on the Reserve system the responsibility of trying to stabilize prices.

Professors Fisher, Commons, Hawtrey, and other monetary theorists have claimed a remarkable success for the Federal Reserve in stabilizing prices between 1922 and 1928, and mourn the untimely death of Governor Strong who they claim was responsible for the policy and its execution. I know of no American students of the Federal Reserve who support them in this position. For instance, Sprague, Bradford, Harris, Reed, Hardy, Steiner, Spahr and Willis contend that any reasonable credit policy during this period would have resulted in a price history similar to the actual one, that the stability was fortuitous rather than manipulated, being effected by a favorable conjuncture of affairs. Bradford compared the per cent of deviation from trend in the years 1900 to 1912 and 1923 to 1928, and found they were 2.17 per cent and 1.92 per cent respectively; this indicates that very little can be accomplished in eliminating cyclical fluctuations in wholesale prices, if this period 1923 to 1928 is an

example of what can be done; it also indicates that this period had stable prices only if compared with a period of inordinate fluctuation, say, from 1914 to 1922 or from 1928 to 1934.

VII. THE COMMODITY DOLLAR

The second device for stabilizing the currency which I designated for discussion is the "commodity dollar." As has been noted earlier in the paper, this is by inference, though not by formal commitment, a part of the Roosevelt monetary policy.

The commodity dollar plan was invented by Professor Fisher about twenty-five years ago and was elaborately described in 1922 in his book "Stabilizing the Dollar." The commodity dollar at the beginning would be a certain weight of gold, not coined, but circulated in the form of certificates convertible on demand into varying amounts of gold bullion known as the "bullion dollar." According to changes in an adopted commodity price index the government would at stated intervals, perhaps every two months, increase the weight of the bullion dollar, if the index had risen, and reduce the weight, if the index had fallen, by a certain per cent. If the index has risen 1 per cent or more, for example, the weight would be increased 1 per cent. If after two months the index still stood at 101 or more, the weight would again be increased 1 per cent. He assumes that a change of 1 per cent a month in the weight of the dollar would ultimately catch up with any price changes, and the index be forced back to par.

The weight of the dollar would not be changed more than 1 per cent at any one time; the amount of gold which the government would require for a dollar certificate would also exceed by 1 per cent the amount of gold which the government would give in redeeming a dollar certificate. This brassage charge, he argues, would defeat dangerous speculation in gold in anticipation of a rise or fall in the gold content of the bullion dollar, by eating up the speculator's profits.

Since the amount of gold required by the government is

adjusted to the commodity index, the scheme is often called an "adjustable seigniorage" or "compensated" dollar. Instead of having a fixed weight of gold with a fixed value, it is, therefore, sometimes called the "stabilized dollar," although this term is not so definitive. Inasmuch as changing the weight of gold which will be given or required for a dollar is equivalent to changing the mint price of gold, the deposit of gold for certificates is equivalent to the sale of gold and the presentation of certificates for redemption amounts to the purchase of gold. The scheme may be described as buying and selling gold at a price varying with the commodity price index number.

The "commodity dollar" scheme is not to be confused with the gold purchase plan employed by Professor Warren after October 22. This was a device to depreciate the value of the dollar abroad and ultimately at home by raising the price offered for gold from time to time. The changes were not regularly made either in time or amount and there was no adjustment to changes in the commodity price index. The Gold Reserve Act of January 29 set the limits within which the price of gold may be varied, namely \$41.34 and \$34.36 in the future; at the present time it is \$35.00. Whether the President intends to try to stabilize commodity prices by varying the price of gold between these limits is not known.

Professor Fisher theorizes that "according as the gold dollar is heavier or lighter, the more or less will be its purchasing power . . . by adding new grains to the dollar just fast enough to compensate for a loss in purchasing power of each grain (and, of course, reversely, taking away gold to compensate for a gain), we can secure a stationary . . . dollar, in terms of purchasing power." "If gold is depreciating relatively to commodities, as shown by a tendency of the index number of commodities to rise, the consequences would be: (1) the weight of the gold dollar would be reduced; (2) the deposit of gold (issue of certificates) would be discouraged, and the redemption of certificates encouraged, both operations tending to reduce the volume of certificates in

circulation; (3) as the gold reserve would fall below 100 per cent some of the certificates in the government's possession would be destroyed instead of being put back into circulation, thus further lessening the volume of certificates. The third of these operations would thus reinforce the second in effecting contraction, would help bring down the rising index number to par, and would obviate, or reduce by that much, the need, at the next adjustment period, of a further increase of the dollar's weight. If gold were appreciating, the opposite conditions would obtain. . . ."

Certain advantages are alleged for this scheme of control: (1) that it is semi-automatic and operable by an administrative board since discretion is reduced to the barest minimum; (2) that it retains gold as a circulating medium and as reserve, thereby making the break with tradition easier; (3) that the persons primarily concerned would be the gold miners, importers, and possibly speculators, the rest of the people being ignorant of or indifferent to its existence; (4) that it is easy to start, and equally easy to abandon if it does not operate satisfactorily; (5) that the loss or gain that comes from changing price levels would under the scheme fall on the government, rather than on the citizens as now; and (6) that all forms of money might be retained in circulation and yet be stabilized, provided that they be kept convertible into gold certificates.

Although the scheme of the commodity dollar has been so eminently sponsored and so elaborately presented and publicised, it has been uniformly rejected by bankers and businessmen and by a goodly proportion of the monetary economists. There are substantial objections which countervail its alleged advantages.

The most-telling objection is that it is based on a theory of price that is unreal and doubtful. There is no necessary direct relation between the price paid by the government for gold and the commodity price level. If the price level is falling and the government counters by raising the price of gold and thus reducing the weight of the bullion dollar, the

number of gold dollars in the reserve is, by mere bookkeeping, increased, but this increase has no direct relation to the price of any other commodity. Changing the price of gold does nothing to raise the price of commodities unless in some manner it puts more dollars, as well as a desire to spend, into the hands of buyers of commodities. A higher value put on the gold reserves will enable the banking system to issue more currency and credit; but whether such an increase actually occurs depends both upon the willingness of the banks to lend and of customers to borrow; without such willingness to employ the greater reserves the price of gold might be changed indefinitely without affecting the purchasing power of the money the people had in their pockets, tills and banks. This problem of getting money and credit into use has been the outstanding problem of the Roosevelt administration; although devaluation to a 59.06 cent dollar increased the gold reserves approximately two-thirds, the problem of getting money into use was not changed. The commodity dollar rests on the principle of devaluation; in fact, it is but a series of devaluations and revaluations. Devaluation increases only the gold reserves in the vaults of the Treasury but not in the hands of consumers and debtors who want to use it. Fisher does not pretend to follow the process through: he says, "an increase of the gold in the dollar will, somehow, increase the dollar's purchasing power. As to the exact process by which this acknowledged result is attained we need have no concern. Personally . . . I believe that this process is through the fact that increasing the weight of a dollar decreases the number of dollars in circulation (not only of gold but of fiduciary money and bank credit)."

The ways suggested on occasion by different advocates of the commodity dollar by which raising or lowering the price level will be accomplished by changing the price of gold are: (1) That a large quantity of gold will buy more goods than a small quantity, that an increase of gold in the dollar will increase the dollar's purchasing power, and vice versa. This has just been shown to be inadequate, for the only way it

could reduce the purchasing power would be to remove dollars from buyers' hands and disincline them to spend, and this it does not do at once or by itself. (2) That holders of gold certificates will present them for redemption when the price of gold is lowered, and will deposit gold for certificates when the price of gold is raised, and the change in quantity in circulation will affect the price level. This would apply only to gold producers and speculators—people who are not buying general commodities, and their influence on the price level would be indirect, slow and limited. Moreover, cautions are taken in the plan to prevent speculators from doing anything. (3) That the destruction of some certificates coming into the Government's possession, instead of putting them back into circulation, will facilitate contraction, and vice versa. This is an inconsequential and supplementary feature which might or might not be used by the monetary authorities. (4) That raising the price of gold will shove up the rate of exchange in New York on foreign centers, thus encouraging exporters, discouraging importers, and adding to the demand from American producers to supply both domestic and foreign purchasers. The experience of last October and November indicated that there was no dependable response of commodity price level to changes in the price of gold although international exchange was badly upset. (5) That apprehension as to the changes in the price of gold will cause a flight from the currency and into commodities, thus raising demand and prices for them. The less said the better about this feature.

Clearly the commodity dollar by itself would not afford adequate means of stabilizing the price level. Among other things it presupposes a sound banking system and its full coöperation with the authorities who operate the commodity dollar. Our banking system is not sound and stable and will not be until its structure is radically changed: it supplies a very undependable foundation to support the commodity dollar. And unless the Federal Reserve is permanently subjugated to control of the monetary authority and is made

to subserve its policy wholly to the purpose of price stabilization, its coöperation cannot be assured. The central bank's function, effectuated through its discount rate, open market operations, and other means, would be to regulate the volume of credit relatively to the gold reserve, which, in turn, would be controlled by the commodity dollar device. But it has been shown that central banks cannot satisfactorily perform this function, especially in forcing credit into use. Someone has suggested that the commodity dollar might strengthen the hand of the Federal Reserve; for example, "in case of a large export of gold threatening a reduction of gold reserve, a provision that a sufficient fall of the reserves (say below forty per cent) must be met by an increase of the price of gold would automatically make such reserves sufficient, so that the Federal Reserve banks need not fear to take whatever action in the open market might be necessary to stabilize the price level." Such a provision, of course, would be an addition to the strict "commodity dollar" plan and might on occasion conflict with its operation.

It would not always suffice to institute remedial action after prices had moved, as the commodity dollar scheme requires; to be efficient the future price should be forecast and the necessary steps taken in advance to prevent price fluctuations of a disturbing nature. Such forecasting would place further heavy responsibilities upon the monetary authorities and their action would be subject to criticism by those who might feel that their legitimate interests were being jeopardized by the policy pursued. Such a case might frequently occur when the necessity arose for applying a check to an incipient boom.

Indeed there are weighty reasons for believing that in actual operation the commodity dollar device would promote inflation rather than stabilize prices. It would be very difficult, for political reasons, to increase the gold content of the dollar during periods of rising prices because of the fact that such action, if it be assumed that it would have an immediate influence on prices, would tend to reduce sharply

the prices of export commodities including many which are of especial importance to agriculture, such as cotton. Furthermore, an attempt to increase the gold content of the dollar when prices are rising would surely be attacked, politically and otherwise, as interfering with, and possibly destroying, what might currently be judged to be "prosperity." And in periods of declining prices accompanying a business recession, prices might not be responsive to a change in the gold content of the dollar because of the absence of a business demand for more credit. Furthermore, efforts to counteract the decline by reducing the gold content of the dollar would tend to encourage unsound uses of credit and exaggerate unhealthy speculative tendencies, but probably would not prevent prices from falling ultimately. The commodity dollar would, in practice, therefore, tend to become a device for changing the gold value of the dollar in one direction only.

This probably one-sided operation of the commodity dollar is, in the present situation, a most unsatisfactory feature, for with gold reserves and member bank reserves literally bursting their vaults the potential inflation is gigantic: it does not need any facilitating devices like the commodity dollar once it is under way. The need is for some powerful restraining forces of which there is a singular dearth just now.

The commodity dollar would not eliminate the price cycle that accompanies and features the business cycle; in fact there are fair reasons wherefore one might expect it to accentuate the cycles, if not cause an additional batch, particularly if it prove, as it surely would, impossible to keep the decision to increase or decrease the gold content of the dollar on a strictly scientific basis, on account of the pressure on Congress and the Administration by interested groups, from time to time, in obtaining relief for such groups or in positive promotion of their interests at the expense of the nation. "Gold has always been recognized in monetary theory as a fundamental *long-term* force affecting the level of commodity prices but, standing alone, it has never been recognized as a

major force capable of bringing about a sharp *short-term* advance in prices. . . . The notion that an abrupt change in the price of gold can, as a single and isolated factor, absolutely dictate a major short-swing change in the price level, is a gross over-simplification of cause and effect in the sphere of money and price" (Edie, *Dollars*, 170, 112). If the commodity dollar has any role to play in the stabilization of the dollar, it must be solely with respect to the secular change, the ten-or-twenty-year swings.

With respect to these secular changes, however, there is doubt as to what the result would be. A long period of rising prices would dangerously deplete the gold reserve. As the weight of the dollar were increased the number of dollars in the reserve would be decreased. Since such chalking up of the weight of gold would occur when the price level is rising fastest and such times are likely to be ones of great economic and political disturbances, both national and international, the government's credit would be weak and it would be difficult to replenish the reserves.

With respect to the operation of the commodity dollar plan in periods of declining prices, it must be remembered that if wholesale prices are used as the guide, which is likely, wholesale commodity prices tend to decline before wages, rents, and the cost of living as expressed in retail prices. And inasmuch as the demand for credit depends upon *all* prices, attempts to prevent wholesale commodity prices from falling, by using the machinery of the commodity dollar, would probably result in so saturating the credit market with funds that many unsound uses of credit would be stimulated, as, for instance, in security speculation. The later and inevitable correction of such inflated security prices would tend to disturb confidence and make it difficult, or perhaps impossible, to cope with commodity price declines, once they have become precipitous.

The Fisher scheme took caution to save the commodity dollar from dangerous speculation in gold by the imposition of a brassage charge equal to the maximum change made in the

weight of the bullion dollar. Dr. B. M. Anderson recently showed, however, that speculation would thwart the plan in every direction, turning attempts to check falling prices into increasing deflation, and efforts to halt rising prices into mounting inflation; that varying the gold content of the dollar was inconsistent with attempts to control credit and would spoil the automatic correctives which exist under the gold standard. His argument runs like this. In case prices are definitely trending downward, so that it can dependably be assumed that the authorities will progressively lighten the weight of the bullion dollar at several successive times, foreigners will withdraw their money from our markets, turning their cash in American banks into gold in order to avoid the loss of 2 or 3 per cent which they clearly anticipate. Speculators will demand gold for redemption, expecting later to turn back the bullion for a greater dollar amount of certificates. People owing debts abroad will hurry to do so while the certificates command much bullion. The general result will be to reduce the money supply of the country, to reduce bank reserves, tighten money, contract bank credit, and accentuate the decline in prices rather than lift them as planned. A similar situation would occur if the price trend were distinctly and dependably upward, except that instead of a bullish operation in gold the speculators and others would be bearish and sell the gold to the government, thus increasing reserves and the basis of currency at a time when it is desired to contract it. Fisher is disposed to minimize the danger of gold speculation and thinks that it is not necessary to protect the commodity dollar against speculation beyond one adjustment period, arguing that prices seldom advance steadily enough and long enough, that the expenses of interest, cartage, storage and insurance would make the speculation unprofitable, and that the speculators would find it difficult and expensive to accumulate sufficient certificates or gold in advance to conduct large operations.

“Altering the gold content of the currency on the basis of internal prices would involve instability of foreign exchange rates. . . . This would be a serious handicap to international trade, but, more

than that, it might lead to competition in currency depreciation. In a world of unstable exchange rates there tends to be constant misunderstanding over alleged attempts on the part of different countries to unduly influence one another's price levels. The frequent accusations that we now hear that this, that and the other country is deliberately depreciating its currency for the purpose of stealing our foreign markets, and that we must depreciate our own to save ourselves, might become a permanent feature of the international situation." (National City Bank, November, 1933.)

Professor Fisher contends that if the United States alone adopts the commodity dollar, she would be emancipated from the involuntary "entangling alliance" of our currency with foreign currencies and of our price level with foreign price levels. Under the gold standard the levels in all gold standard countries are equated: greater independence is desirable. Sooner or later, he believes, the realization of the advantages of stabilization would lead to the general adoption of the scheme among the nations by international agreement.

No national monetary standard can function satisfactorily unless it is adopted by a large majority of the leading nations; and there is no prospect that the commodity dollar scheme will be generally adopted. A commodity dollar which is not adopted at least by the leading commercial and financial nations is not reconcilable with stabilized exchange rates which are so necessary to commerce.

VIII. EXPERIENCE AT STABILIZATION

It is common for advocates of price stabilization by "managed currency" to cite the experience of England and Sweden since 1931 as proof of the possibility of successful management. In September 1931, when Sweden joined the sterling group in deserting the gold standard, alleging necessity, the declared object of the government and Riksbank in their monetary policy was to keep the value of the krona constant in the hands of the Swedish consumer, and to prevent inflation—a diminution of the consumers' purchasing power.

In May 1932 the Riksbank emphasized the necessity of aiming at a rise of the wholesale price level, but only to the

extent that it could be done without raising the cost of living. As the cost of living had fallen more slowly than wholesale prices during the depression, it was thought possible to raise wholesale prices somewhat without disturbing the cost of living. The Riksbank also urged that the function of monetary policy was ultimately to create favorable conditions for carrying on industry and trade and therefore it should not be guided solely by price index numbers, but should also take into consideration such factors as production, warehouse stocks, traffic and the like. This indicates that the monetary policy was changing—the objectives were broadened and dependence on price indexes alone was abandoned.

In April 1933 the government appointed a Currency Commission, which was to criticise the aims and means of monetary policy. The Commission recommended still more emphatically than the Banking Committee of 1932 that wholesale prices should be raised regardless of their effect in other countries, letting foreign exchange rates drift, and urged that monetary policy could only succeed if it were coördinated with the economic and financial policy in other spheres of government. The Government, the Banking Committee and Parliament were all agreeable to these proposals of new policy, which are plainly not for stabilization of the price level.

The means used in effecting the ends of monetary policy were open market operations of the Riksbank in government securities, foreign exchange and gold. The purchases of foreign exchange and gold were with the deliberate purpose of depreciating the external value of the krona, not to raise domestic prices but to minimize the risk inherent in the Riksbank's foreign exchange portfolio. Sweden is a small country, dependent on foreign trade, particularly with Great Britain: this made the operations in gold and foreign exchange more effective than one could hope they would be in the United States where foreign trade is a small per cent of the total trade. To the extent that it was reconcilable with the primary aim of its monetary policy, the Riksbank tried to peg the krona to sterling.

The results of the policy may be summarized as follows:

(1) The commercial banks are very liquid.

(2) The Riksbank acquired large balances abroad, which it was in a position to bring home on need.

(3) The wholesale domestic goods index dropped slowly from June 1931 to March 1933. The price index of imported goods mounted to September 1932, then fell to May 1933, and showed rising prices thereafter. The index of exported goods fell to March 1933 and rose thereafter. Retail prices fell to June 1933 by 15 per cent, and recovered this loss rapidly the next few months. Evidently Swedish monetary and financial policy did not succeed in preventing considerable declines in prices of domestic goods, animal goods and export goods, much less to raise them. Such apparent stability as obtained for all prices was because these declines were somewhat offset by the rise of the index for imported goods, that is, of goods the price of which was determined outside Sweden and was not due to Swedish policy. Since March 1933 prices of domestic and export goods have risen decisively, whereas prices of import goods have fallen somewhat. This compensation again tended to give a semblance of stability for all prices combined.

(4) The depression in Sweden, as measured by unemployment and production output, deepened till the autumn of 1932, then remained unaltered for six months, and thereafter gave way to improvement.

The general conclusion is that even with a small country like Sweden, with conditions by far more favorable to stabilization than prevail in the United States, a stabilization of the wholesale and retail price levels, at least in time of depression, is not possible except when fortuitous circumstances help, and they alone are inadequate. Maybe it would fare better if tried in time of prosperity. Nor has the attempt at "managed currency" been sufficiently effective to lift the country out of the depression. There is at the present time a widespread desire to return to some international monetary standard, preferably the "old" gold standard.

FACTORS CONTROLLING PRICES, DOMESTIC AND INTERNATIONAL

ERNEST MINOR PATTERSON

(Read April 21, 1934)

DURING the last twelve months we have witnessed in the United States (1) the suspension of gold payments, (2) the voiding of gold clauses in both public and private contracts, (3) a reduction in the gold content of the dollar, (4) the purchase by the government of newly mined domestic silver at a price in domestic currency far above the current market quotation, and also (5) the purchase of newly mined domestic gold and foreign gold at a designated price per ounce, this price now being \$35 as compared with the former \$20.67.

No judgment can be passed on these five steps without making certain assumptions. Thus, the voiding of gold contracts raises in many minds various ethical questions upon which it is not peculiarly the function of the economist to pass judgments. They will accordingly be ignored. Instead we shall attempt to give an appraisal in the light of the purpose presumably in the minds of our responsible government officials. But unfortunately these purposes have not been clearly stated and our attempt must be cautiously made and conclusions must certainly not be dogmatic. So nearly as can be determined from official and semi-official utterances, there has been a desire to raise the "general" price level, perhaps to that prevailing in 1926, and then to prevent extreme fluctuations. Apparently wholesale prices are meant but this is not clear, nor can we be certain that a steady price level is contemplated since the various pronouncements have not been consistent on the point. Also the issue is greatly confused by other pronouncements. Thus, there is the statement in the National Industrial Recovery Act that it is "the policy of Congress to remove obstructions to the

free flow of interstate and foreign commerce . . . to avoid undue restriction of production (except as may be temporarily required), to increase the consumption of industrial and agricultural products by increasing purchasing power." In the Agricultural Adjustment Act we find that it is the policy of Congress "to establish and maintain such balance between the production and consumption of agricultural commodities, and such marketing conditions therefor, as will reestablish prices to farmers at a level that will give agricultural commodities a purchasing power with respect to articles that farmers buy, equivalent to the purchasing power of agricultural commodities in the base period. . . . August 1909-July 1914," except in the case of tobacco for which the base period is August 1919-July 1929.

It is unreasonable to expect that there shall be complete consistency in administrative or congressional utterances, especially in such a turbulent year as was 1933, but this lack of consistency makes analysis extremely difficult.

Attempts at restriction of production are, of course, based on the assumption that there is or has been or probably will be what is called "overproduction" of the commodities in question. Usually there is no attempt at clear definition of the term. Apparently it means production in excess of an amount that is viewed as desirable. This is vague.

Nevertheless there is assumed a present volume of production that is to be reduced with a view to raising prices. Reduction of supply has been attempted through actual destruction of some commodities, *e.g.*, pigs and cotton, and by endeavoring to restrict future production. Moreover, this was undertaken not by rigid control but in the case of agricultural products through voluntary coöperation of the farmers.

Judgment should not be too hurriedly or too arbitrarily recorded, but to date there is little reason to believe that such an approach can or will be successful in securing the desired result. There are three explanations that may be offered. First, there are many individuals or groups of producers

involved. There are said to be 2,000,000 producers of cotton. Even though a considerable percentage of them voluntarily agree to a restriction of acreage because they believe such a step advantageous, there are sure to be some who do not and many may even increase the areas planted. Unless a device is found that will compel general reduction, there is bound to be difficulty with a recalcitrant minority. This obstacle has been serious elsewhere, as for example in England, where a minority in the cotton textile industry have prevented a curtailment of output.

Second, the plan ignores the existence of two margins of cultivation—the intensive and the extensive. It provides for a withdrawal from the extensive margin but does not prevent the cultivator from working the limited area more intensively. It is even possible to use the cash benefits secured by retiring a certain number of acres from cultivation to purchase additional fertilizer for use on the reduced area.

Third, and extremely important, is the fact that the size of many agricultural crops depends more on weather conditions than on price. A prospective high price may lead to the planting of a large area, but the crop harvested may be small because of unfavorable weather or vice versa.

These influences seem to have nullified the efforts at curtailment, at least of cotton, and to have led the federal administration to support the Bankhead Bill which definitely restricts the amount of cotton that may be ginned. This new approach does not reach the difficulties just enumerated and there is no reason for being optimistic over the outcome.

In the industrial field there are comparable efforts to raise particular prices by restricting output, chiefly through permitting under the codes many practices formerly illegal. Here, too, there are difficulties. In many lines there is to be found productive capacity in excess of current or prospective market demand. As yet there is little progress being made in eliminating this excess, *e.g.*, by dismantling some of the high cost plants or in preventing an actual growth in capacity by the building of new plants. There are some advances in

price but they must be only temporary unless productive capacity is reduced. To the extent that they may be effective with industrial products without an even greater advance in the prices of agricultural products, the purpose of the Agricultural Adjustment Act will be defeated. Moreover, higher prices cannot be maintained for any commodities, at least for any large volume of sales, except by stimulating and maintaining demand.

Prices are not being effectively influenced by deliberate control of output. Nor has anything very significant been done to stimulate demand. In the spring of 1933 many business men increased their demands because of the widespread fear that prices would rise and because they wished to enter the market before processing taxes and higher labor costs under the codes could become effective. By their increased purchases they drove prices to a distinctly higher level, but the movement soon lost its momentum. By many reliance was placed upon stimulating demand by compelling private industry to pay higher wages. It was argued that workers who received enlarged incomes would make heavier purchases and that prices would respond. This effort also failed. Such influences as were felt were chiefly in the market for consumption goods, but at no time had the consumption goods industries been so depressed as the capital goods industries. The latter could not be quickly affected. Efforts were made to secure results through the Public Works Administration, but these results were meager. Finally, the Civil Works Administration was organized. No matter what its merits as a relief agency its outlays, though large, helped prices only a little.

These failures to influence particular prices by restricting supply or by stimulating demand gave encouragement to those who believed that results could be secured by concentrating effort on the money side of the price ratio as distinct from the commodity side. This view was not held by the vast majority of the professional economists who have specialized in this aspect of the subject, but has had a fascina-

tion for many who have given little or no attention to money or monetary theory. Very few money economists believe that monetary theory or practical experience offers any hope that prices can be controlled in this way.

Nevertheless, the attempt was made. The plan was to reduce the gold content of the dollar, an alteration that was made permissive for the President by the Thomas Amendment of the Agricultural Adjustment Act of nearly a year ago. The reduction from a content of 23.22 grains fine to about 59 per cent of that amount was made on February 1, 1934.

Unfortunately, it has been referred to as "devaluing" the dollar. If by "value" is meant exchange value or purchasing power, it must be stressed that reducing the gold content does not directly or of itself alter the purchasing power. It is true that we do not use gold dollars in payments and can only speculate about the exchange value they would have if they were in circulation. Certainly the paper substitutes for them were not altered in purchasing power because of so-called "devaluation." In fact, most wholesale prices have been a little weak for several months past. It is better not to say that the dollar has been "devalued" but to say instead that its gold content has been reduced.

In one direction there was a definite effect. People with claims on American banks who desired to exchange them for claims on banks in other countries, *i.e.*, those who wished to exchange dollars for pounds, francs or lire, discovered that dollars fell in terms of these other currencies. This was not surprising since these claims may ultimately be realized by the actual shipment out of the United States of the specified number of gold dollars. As soon as it became evident that such shipments, if and when permitted, would be in lighter weight dollars, the foreign exchanges responded. The dollar depreciated to such an extent as to encourage certain exports of commodities from the United States. Demand for these particular commodities, *e.g.*, cotton and copper, was stimulated and their prices rose. The higher prices thus realized have given greater purchasing power to the sellers of these products.

They have in turn increased their buying and we find a strong stimulus to their demand which has shown itself especially in our Southern States. These same changes in the foreign exchanges have tended to check imports, although the influence has been more widely diffused.

But this effect on domestic prices through the foreign exchanges has been relatively slight and cannot well be otherwise, since so small a percentage of American trade is to and from abroad. Altering the gold content of the dollar has had an insignificant immediate influence on the price level. There are certain other effects, however, which should be noted.

First is the fact that our abandonment of the gold standard with its effect on the foreign exchanges has caused a heavy flow of gold into the United States. This movement started in March 1933, amounted to about \$320,000,000 for the balance of that year and is still continuing. During 1933, about \$245,000,000 came from the single country France—of the old weight 23.22. Needless to say, we have not had an inadequate supply of gold and this influx serves chiefly to embarrass those countries which are desirous of remaining on the gold standard. The dollar is fundamentally in a strong position. The demand for dollars is regularly heavy as compared with the available supply—a fact which tends to bring gold to the United States. This has been true throughout the post-war years and gold has pressed in upon us except during brief periods when speculative or other temporary factors have caused exports. Uncertainty regarding American conditions caused such exports just prior to March 1933, when imports were again resumed and on the large scale just described. If our dollar had been reduced in weight by no more than thirty per cent, or perhaps thirty-five per cent, this situation would not have been aggravated. But the threat to reduce by perhaps as much as fifty per cent and the actual reduction in 1934 of over forty per cent was very clearly an undervaluation. Put differently, the offer to pay \$35 per ounce was an offer to pay more than it cost to buy

pounds, etc., with dollars and with those foreign moneys purchase and import gold. Sale to the United States at \$35 per ounce gave a worthwhile profit. This pull on foreign gold has complicated problems in Europe in ways that not only have disturbed their economic equilibrium, but that have definitely lessened our ability to do business with them and thus tend to keep prices depressed.

A second and possibly even more important effect of our attempts to raise prices by altering the weight of the dollar is that although largely futile in the present it may ultimately be very successful. The immediate effects are quite insignificant. But in time business will revive. As economic activities increase, banks will expand their loans and borrowers will spend the proceeds. How far this expansion will go no one knows but past experience suggests that the limit will be determined by the number of dollars held as reserve by the banks and the ratio maintained by them between reserves and deposits.

What about the new limits? Assume no important further additions to the physical stock of gold in the United States. At the end of January 1934 there were \$4,033,000,000 in the Treasury of the United States and in the federal reserve banks. The next day, with no change in the number of ounces of gold but with a new legal weight for the dollar, there were \$6,820,000,000, a gain of \$2,787,000,000. There is no mystery about it. . One can similarly increase the number of bushels of wheat in the country by reducing the number of pounds per bushel. Business is carried on in dollars. The number of dollars is greatly increased. Bank deposits may be expanded far more than before and if there is an active turnover prices may be driven to levels far above those of 1920. All we need as a reminder is the prices we pay today in France in francs or in Italy in lire as compared with 1913.

We may summarize this discussion of domestic prices by observing that prices may be raised by restricting the supply of commodities. Thus far, this method has met with no significant success. Or they may be raised by stimulating

demand. The methods tried to date have failed, although the reduction in the gold content of the dollar may ultimately have very important consequences. There is one other method of stimulating demand for goods and thereby raising prices. If governments print or coin money, or sell their bonds directly or indirectly in such way that they are paid for not out of savings but by bank loans, and if, further, the purchasing power thus brought into existence is spent by the government or given by the government to citizens who spend it, there is brought into existence a demand for goods and prices will rise. And if government deficits are large and continuous, the rise may be to a very high level.

This method of raising prices is not difficult. Instead it is very easy. But as yet no one knows how to restrain the rise within the limits that may be desired. All experience suggests that control is not possible until the rise has gone to such heights that a serious business crisis cannot be avoided. In recent years Germany is the stock illustration. But the less severe experiences of France, Belgium and Italy are not to be overlooked, and there are few who care to see a repetition even of what occurred in the United States during the period culminating in 1920.

Those American economists who have protested so vigorously against our monetary policy during the last twelve months have been concerned because they believe (1) that a lighter weight dollar will permit in time a huge expansion of bank credit that will bring a very high price level, and (2) that the large government deficit will of necessity be financed through heavy advances by the commercial banks. This method of financing tends to raise the price level and also gives to commercial banks an unduly high amount of government paper which is not a sufficiently liquid asset. Unfortunately, each week's news brings further confirmation of their forecasts.

We may turn next to international prices, by which we presumably mean especially the prices of those articles which enter largely into international trade. Or perhaps there is

meant the relationship between the "general" price levels in different countries. Not many years ago, the discussion could have been quite precise and would have been a recital of Ricardo's theory of the flow of the precious metals, the doctrine of comparative costs, Goschen's theory of the foreign exchanges and some formulation of the quantity theory of money. To these four we could, especially in more recent years, have added appropriate references to the theory of purchasing power parity. The five taken together furnish a fairly consistent and convenient doctrinal presentation.

They must today be extensively modified or adapted. Because they assume a large freedom of movement both of gold and of other commodities, qualifications must now be made because of the growth in economic protectionism of all sorts. To the extent that they assume a *laissez faire* regime, allowance must be made for the great increase in governmental controls. If we actually learn how to "manage" currencies, still other complications are introduced.

At present the stage is an intermediate one. With protective devices, including quotas and exchange controls, so numerous, the price of a given article, say wheat, may differ greatly from one country to another. Where controls are so complete as those by Russia, it may be quite impossible to compare prices within that country with the prices outside. There may be little or no ascertainable relation between them. Nevertheless, the monetary and other economic policies of Russia and the similar "planning" activities of other countries have an important effect on prices in the international market.

Time permits a reference to only one. In the past it was possible to demonstrate certain relationships between cost and price in the case of reproducible commodities. In spite of serious difficulties, costs could be calculated at least roughly. But with the growth in the relative importance of overhead costs and with the appearance of so many industries with numerous by-products, the difficulties have grown. Dumping, especially into foreign markets, has become more and more common and of course there is no really logical way of apportioning overhead among different products.

This has been an increasing problem for many years, but with the sudden growth in economic planning on a national basis, the old analyses and the public policies based upon them must be changed. A government like that of Russia (and others in varying degrees) may adjust taxation and the costs of labor and of raw materials in such ways as to make the word "cost" almost meaningless. This and other new procedures facilitate dumping on a large scale. New theoretical issues in the field of money and prices are at once forced to the front and the answers are not yet forthcoming.

Such changes as these have caused a confusion that is greatly aggravated by the economic post-war demoralization that is still with us and by political uncertainties. Long term investments have been discouraged, short term funds have concentrated first in one market and then in another. The foreign exchanges have been erratic. Country after country has suspended gold payments. The result of these and other influences has been an extreme curtailment of international trade. Also there has been a growth of demand for national self-sufficiency or "autarchie" recently modified somewhat by demands for "grossraumwirtschaft" or larger economic areas than the national ones.

Presumably a revival of international trade is to be deemed desirable. Such a revival will be extremely difficult, so long as there is little or nothing in common between the different monetary systems. Enough has been said in this paper to indicate the writer's belief that as yet we know very little about economic controls. There is an almost desperate need for them but our ignorance is abysmal, particularly in the field of money and prices. One of the most serious dangers in the United States is that many reforms in other directions which some think desirable are quite sure to fail of their purpose if we persist in using monetary nostrums.

In spite of the very clear defects of the gold standard, we know nothing better. Adapted and modernized in certain ways regarding which expert agreement can be secured, there should be a prompt and unequivocal return to it at once if we

desire to increase the volume and steadiness of international transactions. This will not solve many other matters but it will remove one of the most serious obstacles to their solution and permit their study in a better atmosphere.

But return is not easy and is now much more difficult than it would be had not the United States so unwisely suspended gold payments, voided gold contracts and engaged in our gold buying activities of recent months. We have greatly complicated our domestic problems and with loss rather than gain to ourselves have imposed on others heavy injuries which react on us. And if we delay definite action until the gold bloc in Europe are forced off gold, our troubles and theirs, both economic and political, will be intensified and ultimate stabilization still further delayed. One of the most disturbing of the current rumors is of a proposal to reduce still more the weight of our dollar.

Perhaps an economist cannot have an intelligent opinion regarding political procedure. He may, however, say with assurance that one of the most urgent economic needs of the moment is for an agreement between the United States, the sterling groups (of which Great Britain is the leader) and the gold bloc of Continental Europe (where France leads) for a return to a modified gold bullion standard.

It is to be noticed that in this paper no attempt has been made to suggest the goals toward which we should aim. I have endeavored to indicate the assumptions upon which the argument has rested. If it is desirable to raise prices, methods should be used which offer some hope of producing that result. If we desire to hold prices at some designated higher level, there is all the more reason for caution in the choice of the methods employed. If we desire to revive the volume of international trade and to lessen at home and abroad such political and military strain as arises from economic causes, then even more care should be used. What the methods should be must be decided not by a blind adherence to the generalizations of the classical or neo-classical economists, for this world is rapidly changing. Yet it is even worse to join

the group of light-hearted enthusiasts who are proclaiming that the past teaches us nothing, or those others who would apply in the field of economics a mechanistic approach which may be helpful elsewhere but which will surely lead us astray in social science.

Measures for raising and controlling domestic prices cannot be dissociated from the world situation unless we wish to undertake the impossible, costly and hazardous task of trying to develop in the United States a self-contained economy. Assuming that our statesmen can overcome political difficulties both at home and abroad, there are some lines of action that can clearly be advised and some broad rules that can be laid down.

1. Particular prices cannot be altered or controlled except by effectively controlling either supply or demand for the commodities concerned. Very slight success in this has been attained thus far, either by governments or by private groups.

2. The concept of a "general" price level is confusing rather than helpful and economists are abandoning it. Attempts to raise or to lower or to stabilize groups of prices cannot succeed under most conditions. To this there are a few qualifications but not many.

3. Price controls through a manipulation of money and credit are not yet possible. Neither *a priori* theorizing nor experience is encouraging. There are a few methods which seem to have a little efficacy at certain times and under certain conditions, but optimism regarding them must be very restrained.

4. The procedure to be followed in the United States can be recommended only on some clear assumption—perhaps that our business structure is to be maintained in about its present form. If this is correct, the most certain and most stable gains will come if we can forget such delusions as those which have been claiming our attention for months past. Price recovery can come best through an improvement in business which in turn must depend upon a return of confidence. If certain general social and economic reforms are

desired, they will be imperiled rather than helped by doses of monetary patent medicine.

5. No matter what has been done in the last year we must start from where we are. Our dollar has been reduced in weight. If that weight could be raised to 65 or 70 per cent of the former weight instead of remaining at the present 59 per cent, agreements with other countries could be more easily secured. A further reduction will postpone such agreements and make them more difficult.

6. Concessions to conditions in other countries would hasten adjustments. Insistence upon our present dollar and upon the present sterling-dollar rate of about 5.18 means a demand upon the British to adopt a unit containing about 78 grains in place of their old one of 113 grains. They will presumably prefer one considerably lighter—somewhere between 50 and 70 grains. No matter what their preference is, we should consider what concessions we can make—either through a heavier dollar or through a sterling dollar rate lower than current quotations.

7. Similar issues will rise with other countries. If we force the rest of Continental Europe off gold, the difficulties will be increased, not lessened.

8. No matter what monetary agreements are or are not reached, even more difficult problems of a non-monetary sort remain for settlement. Their settlement will be greatly facilitated if monetary systems are stabilized each by itself and in relation to the rest. There is every reason for haste and for intelligence if we desire to improve domestic and world business and to lessen the economic strains which bring not only economic distress but political and military dangers.

**ECONOMIC FUNCTIONS OF THE STOCK MARKET AS A
BUSINESS AGENCY AND THE PROPOSED
NATIONAL SECURITIES EXCHANGE
ACT OF 1934**

S. S. HUEBNER

(Read April 21, 1934)

Stock exchanges mean much more to the business and investment world than the mere furnishing of trade rooms and the physical equipment necessary for the marketing of securities. These material requirements, and the frequently spectacular operation of the same, constitute the phases that can be seen from the gallery and which are read about in the newspapers. But that sort of thing is only the mechanical side—the rule of thumb procedure side—of organized exchanges. That side is a mere drop in the economic bucket. The important thing—the soul side of stock exchanges—is the large collective group of services, which is unseen by the eye, and which fulfills the functions of speed, regularity, convenience, economy, and safety in the operation of an indispensable business service, as well as of insurance protection to security owners, creditors and depository institutions. There is no intention, in so brief a paper as this, to discuss all of the numerous technical services of stock exchanges, especially since many text and reference books are available to impart this information. Rather, it is my purpose to outline briefly five outstanding functions of stock exchanges, and with respect to each to indicate the significance of the proposed and bitterly contested “National Securities Exchange Act of 1934.”

I. ASSURANCE OF A CONTINUOUS MARKET

In contrast to the real estate market, non-exchange commodity markets, and over-the-counter security markets, all of which are non-continuous in character, a continuous stock

market may be defined as one which enables buyers or sellers to obtain or to dispose of a security, even in large quantities, at any time during business hours, and at a price varying but slightly from the last previous quotation. Experience shows that such a market can be assured only through a large group of floor speculators, specialists, and odd-lot dealers, operating upon and controlled by an organized exchange. Under normal conditions—and panic conditions are comparatively rare—the daily price range on stock exchanges is surprisingly small, and all interests may count upon either obtaining or disposing of a stock or bond, on an at-the-market order to be executed at once without reference to any particular price, at a very small concession as compared with the last previously recorded quotation.

Using the New York Stock Exchange membership of 1,375 as a basis, it appears that 86 are floor traders for their own account, 289 floor brokers, *i.e.* brokers for other brokers, 128 are odd-lot dealers, 352 specialists, 60 bond dealers and brokers, and only 273 commission firm brokers. The floor traders are professional speculators, acting solely for themselves. They buy and sell only for their own account. Their activity proves beneficial to the public in that they help (1) to give a larger and more continuous market, and (2) to create closer prices between transactions in stocks and bonds.

Odd-lot dealers stand ready at all times to buy or to sell any "odd-lot" of stock (from 1 to 99 shares) at a small fraction above or below the previous quotation for full 100-share lots. As a result of their activities small investors, who otherwise would have a very uncertain and unfair market, are enabled at a moment's notice to buy or to sell any odd-lot—be it one, three, or any other number of shares up to ninety-nine, within an eighth or a quarter of a point of the wholesale price for 100 shares. When approached by the commission firm broker, in the interest of his customer, the odd-lot dealer either sells or buys the security, as the case may be, at the small concession indicated, and never fails in his duty. The smallest investor is therefore given the same continuous mar-

ket as the wealthy. If the odd-lot dealer is called upon to sell the odd-lot, he always sells short, since he does not own the security. He does so in the expectation that his sales will soon amount to 100 shares, at which time he will promptly buy them in the 100-share market, in all likelihood from some floor speculator who is selling short. If asked to buy the security, he will do so in anticipation of having his purchases of odd-lots soon reach 100 shares, when he will go into the 100-share market and sell the same, in all likelihood to some floor dealer. The odd-lot dealer requires a continuous market for himself in order to give a continuous market to the public, and this is possible only if there are plenty of floor dealers and specialists. His profit consists solely of the small difference, and in many instances there is none because of a change in price in the 100-share market, between the price at which he originally bought and subsequently sold, or originally sold and subsequently bought.

Specialists serve much the same purpose as do odd-lot dealers, except that they deal in 100-share lots of a limited number of securities in which they specialize, and which are bought and sold at some particular post on the floor of the exchange. At present they are also allowed to act as brokers for other brokers, but when acting as dealers they may not pretend to serve as brokers and charge a commission. Their service consists in their being ready to trade under all market conditions. Through their speculations they help to create a continuous market for commission house brokers, odd-lot dealers, and all other groups operating on the exchange. Without them fluctuations between sales of certain stocks, particularly some inactive ones, would be much greater than is now the case, and many stocks would likely at times have little or no market whatever. The same general service is rendered by the group of bond dealers in the bond market.

Each group in the market renders its particular service, yet the combined effect of their efforts is a huge increase in the volume of transactions in the market, without any harmful effect on the public, as well as a much greater continuity in

the regularity of the sales. As I have stated elsewhere, all of the component groups constituting the market "depend for success upon short selling and a continuous market. Each group is assisted by the other; all depend upon the continuous character of the market which they themselves help to create; and the continuous market, in turn, is largely the product of speculation and short selling."

Because of the existence of a continuous market the stock or bond is given the quality of liquidity. There is assurance that the security is practically synonymous with money at any particular time, *i.e.* that the security may be converted into cash at a moment's need, or vice versa that cash may be converted into the security. The creditor is assured that he can always protect his loans by selling the collateral before it is too late, *i.e.* the continuous market serves as insurance to the creditor. Constant quotations are also given for the benefit of security owners and creditors. Arbitraging is made possible, *i.e.* the practice whereby certain dealers, known as arbitragers, buy and sell a given security in two different markets at about the same time, when a slight difference in price prevails in those two markets, with a view to clipping the difference, thus making the price for a given security practically the same the world over. The use of stop loss orders is also made dependable. Moreover, because of the two sided character of the organized market—the "bull" or "long" and the "bear" or "short" sides—there is assurance of a much greater degree of stabilization of prices than would be the case if these two contending speculative forces were absent. If the price tends to rise to an unwarranted level, in the light of known circumstances, the bear interest in the market will resist the movement, and vice versa, if prices tend to be unduly depressed, the bull speculative interest will serve as a check to the unwarranted decline. Both of these speculative interests serve not only to furnish a continuous market for either the buying or selling side, but also serve to lessen the abruptness of price fluctuations for each day as well as over long periods of time.

An all important thing to bear in mind is that the public, appearing through commission brokers, does not guarantee the existence of a continuous market in securities, any more than it did in real estate through real estate commission brokers during the past four years. When the news is bad, the public has a decided tendency to take to the tall timbers. Dependability of a market, with a gradual change in price, requires an organized market, enlarged beyond the immediate demand or supply of the public by a large group of professional speculators, *i.e.* floor traders, specialists and odd-lot dealers, free to buy or to sell short at any time and without onerous restrictions. The proposed National Securities Exchange Act recognizes the need for specialists but separates their present trading and brokerage functions by providing that a specialist must elect to be either a broker or a dealer only. With this suggested change we shall not quarrel, although it may force many of this large group to cease serving as specialists, owing to inability to make a decent living, and thus tend to restrict the continuous character of the market. Rather, it would be better to install such a vital change gradually, starting the new practice with a given number of securities and then gradually extending the change to the entire range of listed stocks, in the meantime benefiting from the experience. Fortunately the proposed Act also recognizes the need for odd-lot dealers, and simply requires that they may not also be brokers, a situation already substantially complied with.

But with respect to the general floor trader the proposed Act seems to be harsh. This group has already been reduced greatly since 1914 by the heavy State and Federal transfer taxes, which tend to make the margin between purchase and sale prices so large, in order to give a remunerative living, that many have withdrawn from this special field of activity. Despite this fact, the proposed Act provides that members may be "registered for the privilege of acting as dealers," but "it shall be unlawful for any member with or without the privilege of acting as a dealer (except an odd-lot dealer or a

specialist dealer) while on the trading premises of such exchange to effect any transaction on such exchange for his own account or while on the trading premises of such exchange as a broker to give an order to another member to be executed for his own account." One may wonder where the floor dealer can deal to advantage, if it is not on the floor of his exchange, and this seems to be forbidden. But as is the case nearly always in this monumentally curious Act, the tooth may not be a permanent one—and how much we have heard about the teeth in this Act—because right alongside of this tooth, as of nearly every other tooth in the Bill, there sits some dentist instructed to extract the tooth forthwith if it is in the public interest to do so. So the Bill provides the following with respect to the unfortunate floor dealer, who is denied the floor to deal on: "But where because of the limited volume of transactions effected on an exchange it is in the judgment of the Commission impractical and not in the public interest to deny access to the trading premises of an exchange to a member with the privilege of acting as a dealer for his account. . . the Commission shall have the power on application of such exchange and on a showing that the rules of the exchange prevent excessive trading by members, to permit the members of such exchange to effect transactions thereon for their own account, subject to the rules of the Commission." What an afterthought in economics that section is! Undoubtedly all exchanges will make application at once, and the world may then probably march on as usual. On the other hand, one may wonder whether the framers of this classic piece of legislation have envisaged the difficulty of rejuvenating the floor trader group, after it has once been killed off, should there arise a situation where the volume of transactions in the market becomes too limited.

I certainly expected a lot of teeth in the proposed Bill with respect to short selling. The stock exchange investigation started with a terrific barrage against short selling, at a time when securities had declined to extremely low figures, and when the cause of the decline was assigned by the investigators

to that particular phase of the market. Since that time, however, we have enjoyed an extraordinary bull market in security prices, and the legislative barrage has as a consequence died down. Strange as it may seem, the 63-page Bill has only one sentence on the general practice of short selling, namely "It shall be unlawful for any person . . . to effect a short sale of any security registered on a national securities exchange, except in accordance with such rules and regulations as the Commission may prescribe as appropriate or necessary in the public interest or for the protection of investors." This provision may be a real "tooth," and if so let us hope that the Commission will be a good dentist.

2. EFFICIENT FINANCING AND THE INSURANCE OF CREDITORS

It is said that about 80 per cent of business is conducted on credit. But it is important to bear in mind that credit operations are not limited to commodities, real estate, and equipment. Our vast mass of outstanding securities must also be supported by credit, or a terrific devaluation will result. In 1929, the market value of stocks and bonds listed on the New York Stock Exchange totalled \$137,000,000,000 or about 30 per cent of the estimated national wealth of \$400,000,000,000. Since that time it may be assumed safely that the dollar value of the national wealth has experienced a decline approximately equal to the decline in dollar valuation of the securities listed on our exchanges. If we add the list of securities on the New York Curb Market and the twenty-one other stock exchanges of the country, it would seem reasonable to conclude that stock and bond certificates listed on exchanges represent approximately 40 per cent of the nation's wealth.

Such an enormous amount of wealth is not owned outright by the holders, but is outstanding in large part because of credit support. Owing to the existence of a continuous market, bankers and other creditors are willing to enlarge greatly the volume of credit on securities (*i.e.* are willing to accept a much smaller margin—a customary 20 per cent—

as between market value and size of loan), since they know that the collateral can be sold at a moment's need in a dependable market which fluctuates but little in the course of an hour or even a day. Despite this credit situation, handled in the main directly through brokerage offices and by them indirectly through the banks, the proposed National Securities Exchange Act of 1934 makes it unlawful for any member of a national securities exchange, or for other lenders with a few stipulated exceptions, to extend credit on any security registered on a national securities exchange (other than an exempted security) "in an amount exceeding, whichever is the higher, (1) 40 per cent of the current market price, or (2) if the security has been traded in on a national exchange for a period not less than 36 months, 100 per cent of the lowest price at which such security was sold during the preceding thirty-six months but not more than 75 per cent of the current market price."

This section of the proposed Act has been more bitterly contested than any other, chiefly on the ground that it invites a very serious devaluation in equity securities. So strenuous became the opposition, that the Bill was changed to provide a dentist for the extraction of the tooth whenever necessary. The 60 per cent margin requirement was retained, but to the Federal Reserve Board was assigned the work of dentistry so that "where it deems such action vitally essential to the accommodation of commerce and industry and with regard to its bearing on the general credit situation of the country, it may by rules and regulations permit lower margin requirements for particular securities or transactions or classes of securities or transactions and for particular periods." Evidently heeding the danger of deflation, the proposed Bill was further changed by stipulating that the margin provision "shall not apply on or before January 31, 1939, to any loan, renewal, or extension thereof on any securities registered on a national securities exchange, made prior to the enactment of this Act." Serious criticism has continued, the aim of which is to leave the fixing of margins to a designated administrative body, to act from time to time as the situation may re-

quire, instead of prescribing the exact margin figures in advance in the Act itself.

The shortcoming of this all important section lies in the fact that it does not and cannot distinguish between those who are financially able, and reasonably intelligent in such matters and that large group which is utterly incapable of expressing any sane judgment with respect to equity securities, which possesses no financial strength of a worthwhile character, but which nevertheless is imbued with the speculative instinct. One margin requirement—one glove—is to be strapped upon two totally different hands. The motive of the framers may have been good, namely to protect the ignorant public, who, we shall all agree ought never to speculate, and certainly not on margin, by making it difficult to speculate with borrowed money. Despite its ruinous effect upon credit requirements, we might still support this novel legislative provision if it would accomplish what its framers had in mind, namely the protection of the unsophisticated members of the public who take a fling in stocks, about which they know absolutely nothing, just as they do in real estate, commodities, non-exchange securities, and in other directions, instead of placing their thrift and investment dependence with depository institutions under trained managements.

But will the provision accomplish its intended purpose? One cannot help but feel that the framers were more theoretical than practical. The speculative instinct in man, amounting to gambling on the part of the unknowing, is seated deeply, and can no more be cured by legislative endeavor to raise the margin fence, than could the liquor evil be cured by prohibition legislation. The futility and harm of a 60 per cent margin requirement is apparent, when we reflect that it will drive the small speculative group from the good high-priced shares to the low-priced inferior shares, thus increasing their chance of loss. If such a person cannot pay \$6,000 down on 100 shares worth \$100 a share, he will then turn to \$2 and \$5 shares, selling at that price because they are no good, because 100 \$5 shares will cost only \$500 and 60 per cent of \$500 is

only \$300. Certain corporations may also be tempted to split their present high-priced shares into a large number of low-priced shares. With the speculative instinct deeply seated, a 60 per cent margin provision will simply drive the would-be speculators from one pasture to another, *i.e.* from the stock market to real estate and commodities, where the speculative hazards are really greater than they are in the security market. Moreover, driven to obtain the funds necessary to meet the deep seated instinct, there will even be a tendency to withdraw funds from life insurance, savings banks and building and loan associations. Yes, if speculation in all kinds of markets were absolutely forbidden to this class, under dire penalties, we should probably witness the substitution of other activities even worse. Speculation and margin dealing are economic necessities, but they should be limited to the right class of people, just as is the case with any other legitimate economic activity. No one is more disposed than I to wish the complete elimination of the great rank and file of wage and salary earners from margin dealing in stocks and commodities, but I feel that the only effective way is through education, which while the longest way around will be found to be the shortest way home. Let us hope, therefore, that education in such matters as conservation through thrift and sound procedure of investment may be made a part of the curriculum of our educational system.

3. CLEARING OF INFORMATION AND DISCOUNTING

Speculative exchange markets are pivotal centers where thousands of minds concentrate to express their opinion concerning prices, in the form of actual sales. Exchanges, in other words, are agencies for the collective expression of human judgment concerning the present and prospective values of securities. Speculation deals primarily with the future and only incidentally with the present or with the past. Corporate stocks constitute so large a proportion of the world's wealth, and represent such a variety of industries, that a marked advance or decline in the general level of prices is the

surest indication of coming prosperity or depression. And the all important fact is that such changes in the price level precede, *i.e.* discount the event, and do not follow or happen concurrently. As I have stated elsewhere:

“It is the failure to understand this fundamental law of price movements that has been the cause of enormous loss to the unknowing, whose judgment is based upon what is seen, read and heard at the time. Organized market price movements have a tendency to go opposite to that which is seen and heard in the present. When the favorable news, whether it be big crops, large earnings, devaluation of the dollar, etc., becomes common information, it has been discounted by the market; similarly when the bad news is apparent it has likewise been discounted. It is only natural, therefore, that the rank and file should regard the stock market as a most incomprehensible affair, always going contrary to what is so perfectly evident to them at the time. It is quite natural that they should always blame their losses on the working of the market rather than on themselves, although in reality their trouble is due to their failure to realize that major market movements anticipate and therefore precede the future. If business preceded the stock market, everyone could become rich. All that would be necessary would be to observe the progress of business, easily ascertainable from current published statistics, and then, when a decided improvement in business is recorded, merely place buying orders in stocks and subsequently cash in on the operations, since, under this fallacious theory, the stock market will rise when business has improved. The truth is that business follows the stock market and does not precede. With respect to our industrial cycles, the past has shown that the stock market had its violent decline during the year of greatest prosperity, marking the end of the industrial cycle, and its violent rise during the period of greatest blackness marking the end of the cycle.”

With many of the reasons stated in the introductory economic section of the National Securities Exchange Act, as justification for governmental regulation of exchanges, we shall not take issue. But one of these alleged reasons we cannot disregard, namely that “national emergencies, producing widespread unemployment, dislocation of trade, transportation and industry . . . are precipitated, intensified, and prolonged by manipulation and control of security prices and by

excessive speculation on exchanges.” Nothing is said of the land and real estate booms in Florida and California and throughout the country, nothing of the unwise extension of billions upon billions of credit through the installment plan of buying, nothing of the unreasonable boom in manufacturing, mining, and every line of business, nothing of the flotation of billions upon billions of non-exchange securities, and nothing of the disastrous credit effects of the recent World War. As I testified at the stock exchange hearings in Washington:

The factors which caused the stupendous and forced liquidation in practically all types of values during a business depression—whether represented on exchanges or not, and usually to a degree fully as great in values not determined on exchanges—are fundamentally economic and inherently associated with the economic nature of man and would exert substantially the same ultimate influence by way of price declines, irrespective of the presence of speculative buying or short selling. Speculative markets are not responsible for the unwise buying and the unwise credit commitments and the world forces that cause our periodic economic ups and downs. Speculation, whether on the long or the short side, merely reflects the bearing of those fundamental factors upon values. It passes its collective judgment upon those factors and seeks to appraise them in the form of price quotations. The speculator, be he buyer or short seller, is interested solely in the correctness of his judgment with respect to the future course of prices. He aims to anticipate the future with regard to business conditions. From a practical standpoint professional speculators, as a group, are anxious to see prices higher rather than lower—at least sufficiently high to please the community—because they know that unusually low prices spell public criticism and endless trouble for exchanges. When entering upon his commitments, the speculator has no reason to prefer declining over rising prices. Either side of the market will net him the same gain—furnish to him the same motivating force—and his only problem is to consider and interpret facts and to appraise correctly the factors which govern the future movement of prices.

4. LISTING REQUIREMENTS

Before the stocks and bonds of a corporation can be dealt in on the floor of an exchange they must be “listed,” which

means that they must be approved by the exchange from the standpoint of prescribed standards of legality security and workmanship. Listing constitutes one of the outstanding functions of an organized security market. It is a distinct protection to the public in that the exchange has demanded from the corporation and has made careful inquiry into the following: The organization of the corporation, the nature of its securities and the description of the property, the engraving and protection of the security against counterfeiting, the printed text of the security, compliance with all of the exchange's rules pertaining to transfer and registry, and compliance by the corporation with all of the agreements imposed by the exchange. A popular misconception is that when an exchange admits a security to its list it therefore recommends that security for favorable consideration. This conception is entirely wrong. The exchange expressly disclaims any intention either to recommend or to condemn any listed security from the standpoint of price. Its function is to make available to the world an open and continuous market in such stocks and bonds as meet its prescribed standards of legality and form. These standards are very high, if the New York Stock Exchange is taken as our example, but the question of price is left entirely to the decision of the individual investor or speculator. No other course is practicable, because who could appraise each year the value of America's numerous corporations which have listed their securities on an exchange. And even if this were attempted, the appraisal could be little more than an estimate, and since all business is speculative with its fortunes varying greatly from time to time, such estimates would be of only temporary value.

Nor has the exchange power to act unduly as a censor of the numerous corporation managements and to dictate unduly their actions and business policies. The proposed National Securities Exchange Act continues the work of listing with the exchanges, but prescribes that the Federal Trade Commission must also give its approval to the security after the exchange has approved the same. The listing requirements in the pro-

posed Act do not add anything to the requirements already prevailing on the New York Exchange, except in one particular, namely along lines which a private exchange could not ask and which can only be enforced in pursuance of a statute. Thus the proposed Act provides that the corporation—the issuer—whose securities are listed on a national securities exchange must agree not “to lend funds at the money post of any exchange, or to any member thereof, or to any broker or dealer, except as the Federal Reserve Board may prescribe”; that “the issuer must state the remuneration, as well as the interests in the securities and material contracts with the issuer and affiliates, of all directors and officers and principal security holders and underwriters; also the remuneration of all others exceeding \$20,000 annually, including bonus and profit sharing arrangements, service contracts and options in respect to securities; that every officer and director and other person owning more than 5 per cent of any class of equity security must report the amount owned, and any change in ownership within ten days after the close of each calendar month; and that “any profit realized by such beneficial owner, director or officer from any purchase and sale or sale and purchase of any such registered equity security within a period of less than six months, unless such security was acquired in good faith in connection with a debt previously contracted, shall inure to and be recoverable by the issuer, irrespective of any intention on the part of such beneficial owner, director or officer in entering into such transaction of holding the security purchased or of not repurchasing the security sold for a period exceeding six months.” Whatever may be thought of such requirements, it is apparent that a private stock exchange cannot well exact the same, and that if they are deemed necessary the requirement must be by statute.

5. DISCIPLINARY CONTROL OF CONDUCT AND PRACTICES

Where buyers and sellers are so widely separated territorially as they are in the security market and where both must

leave the execution of orders to their respective brokers, it is easy to understand how a lack of rigid control of business ethics and practices would lead to much unfairness and fraud. One of the outstanding services of exchanges is the maintenance of just and equitable principles of trade. To this end stock exchanges formulate and rigidly enforce standards of honesty and equitable usages upon members, their partners, branch office managers, and practically all employees of members, as well as with respect to the admission requirements for membership, rules against fictitious transactions, fraud and manipulation, supervision of advertising, market letters and other publicity, and supervision of the execution of transactions and the conduct of accounts. In addition, exchanges have also disciplinary rules designed to cover fraudulent or unbusinesslike practices which may not be specifically prohibited, "a standard unknown to any system of law."

Comparatively few cases of dishonesty arise in transactions effected for the public by members of exchanges. And with respect to the solvency record of stock exchange members, the number of failures is extremely small. In fact, during this memorable depression about the safest place in all the world to have had one's money on deposit was with a New York Stock Exchange house. With respect to this Exchange, the percentage of failures to the outstanding total membership was zero in 1926, only 0.09 per cent in each of the years 1927 and 1928, 0.16 per cent in 1929, 0.44 per cent in 1930, 0.59 per cent in 1931, and 0.21 per cent in 1932. Compare, for example, the failure record of $\frac{1}{5}$ of 1 per cent of the membership in the severe depression year of 1932, with a failure record during that same year of 7.60 per cent of all United States banks and of 4.49 per cent of all national banks. For all banks the relative number of failures was nearly 37 times as large and for national banks nearly 21 times. And we are also advised that on the average, despite the fewness of New York Stock Exchange house failures, settlement with creditors in about half of the instances has been for 100 cents on the dollar.

A large portion of the National Securities Exchange Act is devoted to the prohibition of fictitious transactions, matched orders, the circulation of false rumors, false advertising, the making of false statements, the use of various manipulative devices, and the commingling or lending of securities without customers' consent. Little is added by this legislation as far as the New York Stock Exchange is concerned, because that Exchange already has its severe disciplinary regulations with respect to such dishonest practices. The vast mass of crookedness in the security market has been perpetrated outside of the jurisdiction of our stock exchanges. It is in this respect that the "over-the-counter markets" section of the National Securities Exchange Act will bring great good if the regulatory body will be sane and reasonable in its rules and regulations. This section provides for the registration with the Federal Trade Commission of all dealers or brokers making or creating such a market and makes it "unlawful for any broker or dealer, singly or in concert, to establish an over-the-counter market in contravention of the rules and regulations as the Commission may prescribe."

LIBERTY, PROPERTY AND RECOVERY

JAMES T. YOUNG

(Read April 21, 1934)

THOSE who oppose the Recovery Program have repeatedly pointed to the heavy sacrifice of individual rights which it involves and to the constantly narrowing sphere of personal liberty which seems an inevitable part of the plan. In order to gain a fairer picture, however, we must appreciate that in place of every right or immunity which the Program limits or destroys, it definitely claims to set up a new right or benefit, of broader social import. It claims to offer in a real sense "New Rights for Old." We must also consider that this process of exchanging rights is not recent but has been taking place steadily ever since the invention of the steam engine.

Thus far no effort has been made to compare the existing rights and those which it is proposed to put in their place. Let us now make this attempt, using the term "rights" not in the juristic sense but in a slightly broader meaning, and setting down in parallel columns first those which the Program aims to create and next those which it limits or destroys.

RIGHTS PROPOSED

1. Society's right to a system of production adjusted to demand, by government authority, thereby reducing extreme fluctuation.

Illustrations—Limitation of (a) plant capacity and (b) of actual amount produced, in farming and manufacturing.

2. Right of business self government to control recalcitrant minority and thereby protect markets. Price Fixing.

RIGHTS DESTROYED

1. The individual's right to produce in such quantity, quality, price and terms as he wishes. Flexibility and freedom of management.

2. Right to market goods on conditions which suit the producer's and distributor's needs at the moment.

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| 3. Right of worker to be free from employer's influence in selecting a union. | 3. Right of manager to employ such persons as he may select for the work to be done, including right to set up company union, based on knowledge of and interest in local conditions. |
| 4. Right of closed shop union to persuade or coerce all workers into membership. (Wagner Disputes Bill, Session, 1934.) | 4. Right of worker to join or not to join any organization. |
| 5. Right of worker to unemployment insurance without cost to himself. (Wagner Unemployment Insurance Bill.) | 5. Right of employer to set up voluntary systems of unemployment insurance, or not to do so, as he prefers. |

Next we shall consider a group of rights which more distinctly bear upon the ownership and use of property.

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| 6. Social right to require surrender of gold as a currency basis and of gold bullion,—to pay for such gold in paper money,—and to secure and confiscate a profit of 40-50 per cent on the gold so obtained. | 6. Right to hold any form of nondangerous property and to enjoy the profits and fruits thereof. |
| 7. The social right to scale down government debt obligations to lower levels, thereby facilitating payment. | 7. Right of creditor to full return of sums loaned to government and to payment in the terms on which the loan was made. |
| 8. Right of bank depositors to added safety created by compulsory deposit insurance without limit of liability of the insurers. | 8. Right of bank shareholders to know the nature and extent of the obligations imposed by insurance laws. |
| 9. Right of government as social agent to force additional capital on the banks, the purpose being to ease bank credit. | 9. Right of bank to determine its own capital. |
| 10. Right to lower utility rates and, to this end, to use the tax power to set up government ownership and operation of enterprises which will compete with private utilities. | 10. Right to engage in any legitimate business. |
| 11. Investor's rights to greater security in stock and bond purchases. | 11. Right to sell stock of any legitimate incorporated company. To secure a loan on stock collateral from anyone. To join or not join the membership of the security exchange. |

Next we consider a brief list of procedural rights.

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| <p>12. Right to speedy, effective administrative action in the social interest.</p> <p>13. The right to a genuinely national solution of national problems, or differently expressed, to uniform national action unhampered by local, regional or state interference.</p> <p>14. Right of the government as a social agency to present such facts as it pleases for popular consideration.</p> <p>15. Right of government to license industries and enterprises. Newspaper Code and New Jersey Laundry Code, Instructions, March 19, 1934.¹</p> | <p>12. Right (a) to thorough discussion and ventilation of all important proposed laws by deliberative bodies; (b) to protection against hasty or secret administrative action without hearing and (c) to reasonable certainty and ascertainable knowledge of the law.</p> <p>13. Right to local self government based on greater familiarity with local conditions and subject to local popular control.</p> <p>14. Right of the citizen or the newspaper or other agency of popular information to secure any data about the government from any source. (Federal Reserve Board Bulletin, October, 1933.)</p> <p>15. Freedom of speech and of the press.</p> |
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As we review the broad scope and the penetrating character of this vast exchange of rights, the first question that arises is—How shall we decide on the wisdom of this vast exchange as a whole, or of each separate feature of it?

¹ The Laundry Code Administrator, acting under the authority of the New Jersey Recovery Act, on March 19, 1934, issued a binding regulation for the entire laundry industry containing such statements as—

“They must not refer in advertisements to the prices as something which has been imposed upon them by the Code Authority, or make use in advertisements of such phrases as:

The Code Authority
The NRA has fixed prices
The lowest Code Prices allowed
We cannot be undersold
We charge these prices by order of the Code Authority
By order of the Code Authority, no one may sell at cheaper prices, etc.”

. . . Such terms as the following are prohibited:

Since prices are all the same why not patronize a good laundry?

It costs no more to patronize: this *oldest*, or *largest*, or *best* laundry.

You can now get . . . laundry quality at no extra cost—why be satisfied with less?”
etc.

If such a question were to arise in the natural or physical sciences, the answer would be simple—we should make an extended series of observations in which every effort would be put forth to eliminate chance error, bias, or distortion. From the data so gained we should seek to trace by induction any lines of continuity or concurrence and to derive from these inductions our program of action. In government there is a complication due to the need for securing popular assent. Government action may be dissected into four familiar parts—the creation of an emotional background of pressure for action, the organization of this pressure by propaganda and pressure groups, the technique of bill drafting by legislative bureaus, and, somewhere in this procedure, a fact finding process which will ascertain what the essential data are and what the effects of a given law or change of policy would be. This last process requires that government shall provide a vast series of impartial fact finding bodies or authorities whose vital function shall be to supply the necessary data for government action. Let us now place our question in slightly different form. What materials have we for an intelligent decision on the wholesale exchange of rights just described? The answer is—None.

For many generations both parties and all groups and factions have built up emotional backgrounds, have perfected legislative techniques and have organized effective pressure groups but have omitted the fundamental, essential part of the whole problem, the collection of data. With a few exceptions such as railway laws, the Federal Reserve System, and others to be presently noted we have acted on slogans and catch words. In the immense exchange of rights now proposed both sides are continuing this same primitive method. They offer us pictures, allegories, epithets and shibboleths but no comprehensive impartial data. In this, be it repeated, the Recovery Program is not peculiar but is only following the customs of former times. There is, however, an important distinction in that the Program proposes comprehensive economic planning. How can such planning be undertaken

with our present factless methods of policy-forming? Nevertheless, we are cheerfully trying to decide the issue by the use of such terms as regimentation, collectivism, the death of individualism, democracy, dictatorship, recalcitrant minority, chiseller, business self government, yellow dog contract, and that choicest of all vague terms, Emergency. Those of us who oppose and those of us who favor the exchange of rights, resort with equal freedom to mouth-filling and heartwarming generalities which are supposed to carry conviction among the electorate. At this moment we are not prepared to decide the most important questions which have arisen since 1860.

Since this conclusion, if correct, carries with it broad implications as to policy, let us select the most important present and recent measures and ask as to each what its factual basis has been. For many years we have had a tariff commission, often composed of well qualified men of both parties. With cordial unanimity, however, each party when in the majority in Congress has started its process of tariff-making by carefully laying to one side the reports of the commission. How has it been with our prohibition policy? It would add much to the gaiety of science if some research worker outside the social field were to set down without comment the record of our national law-making on alcohol. His report would run about as follows—After more than 70 years of agitation but with no comprehensive collection of data we adopted a constitutional amendment and buttressed it with enforcing laws. The final decision was made in the guise of an emergency measure to help win the War. Hardly had we done so when a deluge of anti-prohibition slogans was let loose. Bootlegger, gangster, corruptionist, youthful dissipation, capitalist and sectarian bigotry and a host of other epithets flooded the country. Only in the closing moments of Prohibition was a government committee of scientists, under the chairmanship of a distinguished member of this Society, appointed to ascertain the physiological, financial and social facts of the alcoholic beverage business. But the emotional build-up for repeal had already progressed so far and so fast that the law

maker could not wait for facts. Submitting repeal to the voter under a barrage of slogans, it was decided that the full control of the manufacture and sale of intoxicants should be returned *to the states* without reservation. We thereupon set up the most complete *federal* control of manufacture in our history. At no step in this 80 year drama, the end of which is not yet, have we acted upon a comprehensive collection of scientific data. The regulation of the sale of securities touches a vital spot in our structure. Here we might naturally expect an inquiry into the peculiar needs of certain types of industry for long term capital and the extent of these needs. If they reached a considerable magnitude, the law maker might be expected to insist that the regulation of security sales should be so drafted as not to threaten the supply of long term capital. This is a faithful description of what has not been done.

Let us next take the two most controversial measures before the 1934 Session of Congress,—the Stock Exchange Control and the Labor Disputes bills. With no criticism of the content of these measures let us ask solely—What is their underpinning of facts? Again the answer is startling. The emotional build-up for a drastic regulative law on stock markets was exceptionally strong. It was a by-product of a Senate Committee investigation of bankers. For our purpose only a few of the principal stages need be recalled. One was the cross-examination of the leading banker of America as to his income tax payments. The papers of the nation carried heavy headlines, “J. P. Morgan Evades Income Tax.” This was not true. Later the more substantial and truthful disclosures in other directions increased the public indignation. Next, it was found that during the speculative orgy of 1929 many corporations lent their surplus funds in the money market and that this market was utilized by stock speculators. Again heavy headlines, “Corporations Lend Twenty Billion Dollars For Stock Speculation.” This likewise was untrue. The figure of twenty billion was secured by the simple process of multiplying each loan by the number of times it had been renewed. By this time the public mind was in a tumult of

indignation. The emotional build-up was good. The drastic control bill which was then produced was fully up to the heated state of public feeling. A final touch, extracting the last bit of emotional profit from the situation, was given when the Senate committee counsel, upon the strength of the disclosures, ran for elective office in the City of New York while conducting the investigation.

The Labor Disputes Bill according to repeated statements by its sponsor was designed to abolish the company union, facilitate the closed union shop in all branches of industry, revive the injunction but only against the employer, and give enforceable powers of decision to the National Labor Board but only against the employer. It created 5 new offenses for which employers alone might be heavily punished. How were the facts to be found on which such a drastic measure should be based? By a simple contentious hearing before a congressional committee. But how could such a hearing, in which each side presented only its own case and sought to prove its own contentions, produce an impartial accumulation of facts? The answer is found in the words of the Chairman of the Senate Committee—"Thus far (the middle of the sixth week of hearing) we have had a great diversity of philosophies but surprisingly few facts." The hearing closed a week later.

Let us pause here to consider two objections which may be offered at this point. Is not this fundamental weakness of factless policy-forming due to the general backwardness of political science? The inquiries of Reinsch, Luce, McCarthy, Parkinson, Lloyd-Jones, Logan, Herring, Odegard, Bernays and a host of others have set forth both the processes of law making and the pressure groups which influence them. There is abundant scientific knowledge on how to draft and execute legislative policy.

Again, it is urged that the very nature of law-making in a democracy exalts the importance of popular emotion. Without the pressure of such emotion, laws cannot be passed. The answer is that in certain outstanding instances we have clearly shown that we can find facts and that we can use them

in forming public policy. If our people were more freely supplied with facts, they could act accordingly. The Federal law of 1913 setting up the Reserve Bank System was passed after a complete and exhaustive accumulation of data upon which a broad campaign of popular education was then based. The Act corresponded to a thoroughly convinced public opinion. The Interstate Commerce Commission in its numerous reports and inquiries has repeatedly furnished the data required for vital improvements in the rail transport system. In this it has been joined by the researches of the U. S. Chamber of Commerce and is now being supplemented by the fact-finding efforts of the Federal Railway Coördinator. The Federal Trade Commission has made many inquiries on highly controversial matters, the results of which are invaluable for law-making. Good examples are its studies of price protection in retail sales, the distribution methods of the meat industry, the practices of public utility companies, etc. Other branches of the Government might be named, as exceptions it is true, but yet in sufficient number to show beyond doubt that the practice is feasible and that in vital matters both the people and the law maker can be persuaded to act upon it. Our state and local governments are now being prepared for intelligent action by local, regional, and national studies of community planning.

In general answer then to the substantial question with which we started, may we record the following impressions:

1. The decision on the sacrifice of all or any part of a long series of hard-won rights and the benefits to be secured by the change is a decision on vital questions of policy for which we need abundant, impartial and convincing evidence. To make such decision we need within the government itself some comprehensive adequate machinery for securing, organizing and presenting to the people full data on all essential questions of policy. In order that such data shall inspire confidence they must be as far as possible non-partisan in both source and treatment.

2. Since under any system of regulation the economic

barometer may still be subject to some rise and fall in the future, should we not avoid the excessive cost of uninformed experiment by starting now our fact-finding for future emergency needs and for economic planning?

3. The impetus towards fact-finding and towards its use in public policy-forming must come largely from the scientific world. This probably involves a new conception by the scientist of his place in public affairs. As a citizen he might command a prestige and influence out of all proportion to his numerical strength, he might as a citizen insist upon a modern technique in the handling of public questions.

PROBLEMS OF GOVERNMENT OWNERSHIP AND OPERATION OF THE RAILROADS

EMERY R. JOHNSON

(*Read April 21, 1934*)

THE economic problems created by the current business depression are many, and by no means the least of them is the present situation of the railroads and the railroad policy that should be followed by the public and the government. In deciding upon that policy the first and fundamental question to be answered by the people of the United States is whether to continue private ownership and operation of the railroads or to look forward to and prepare for government ownership and operation. The advocates of the nationalization of the railroads have spoken and doubtless will have more to say.

Among such advocates is the highly-esteemed and exceptionally well-informed Coördinator of Transportation who in his report of January 20, 1934, upon the Regulation of Railroads expressed the opinion he has long held that "Theoretically and logically public ownership and operation [of the railroads] meets the known ills of the present situation better than any other remedy"; that it is his "belief that such ownership and operation will be the ultimate solution of the railroad problem." While Mr. Eastman is "not now prepared to resort to public ownership and operation . . . for the principal reason that the country is not now financially in a condition to stand the strain of an acquisition of these great properties," he believes the time will come when "private enterprise and capital will not be able to carry on successfully" and that government ownership and operation of American railroads will follow.

A pessimistic note of apprehension has recently been sounded by Dr. Virgil Jordan, President of the National Industrial Conference Board who stated in addressing a

joint meeting of the Traffic Club of New York and the New York Board of Trade, March 14, 1934, that

"The question whether the responsibility for the development of the American railroad system is to rest in governmental or in private hands is crucial for the future of private enterprise in every field of economic activity in this country. Yet there is so little understanding of the situation and such great indifference towards it today that it is safe to say that by 1940, or not long after, our railroad system will be wholly in the hands of the government."

PRESENT STATUS OF THE QUESTION

Is government ownership and operation of railroads in the United States only an academic question or is it actually a practical one upon which the light of facts needs to be thrown to enable the public to act intelligently in shaping its future policy? As Mr. Eastman says, "there is now little effective support in public opinion for public ownership and operation" of railroads by the United States government; but even a cursory survey of the present situation indicates the presence of combustible elements that may conceivably start a fire that might develop into a conflagration.

These are days of rapid expansion of the functions of government. The progressives are in the saddle in Washington with fair prospect of remaining so for several years to come. Collectivism is at least temporarily narrowing individual and corporate action and discretion with unprecedented rapidity, and it may be that the senators and others who are so successfully spreading the practice of government ownership and operation of hydroelectric and other public utilities will soon be urging a like policy as to railroads. Personally I believe we shall, ere many seasons have passed, slow down our political engine and throw it into a more conservative gear; but there are not a few who seem to be certain that the engine will continue in its present high gear.

Aside from the effect that the general political trends may have upon public thought regarding government ownership and operation of railroads, many persons will doubtless assume that the government has taken a step toward ownership

and operation by loaning seemingly large sums to the railroads to enable them to meet maturing capital obligations and fixed charges, to acquire necessary or desirable equipment, and to carry on or complete important structural work. As a matter of fact these loans have been made to lessen somewhat the burdens of railroad employes and creditors, to enable private ownership of railroads to weather the stormy days of business depression, and thus to make the government assumption of railroad ownership and operation unnecessary. Not realizing this, or because they prefer to think otherwise, some people have, without giving the matter careful study, assumed that the present financial, traffic, and other difficulties of necessitous railroad companies will continue, that prolonged government aid will be needed, and that the result will be—and fortunately so—the taking over of the railroads by the government.

Another factor in the present railroad situation that cannot be ignored is the policy being followed by railroad labor organizations. While not advocating government ownership and operation, organized railroad labor is seeking legislation that if enacted would bankrupt private railroad management and make government assumption of railroad transportation necessary. There is little immediate prospect of such legislation; but one can but remember how easily organized labor brought about the insertion in the Emergency Railroad Transportation Act of June 16, 1933, of provisions that have practically nullified the anticipated economies in railroad operation that were to be accomplished through the good offices or the authority of the Coördinator of Transportation. It may be that organized railway labor will conclude that the surest way to secure a six-hour day, to shorten the length of trains and thus increase the number of employees, to obtain unemployment insurance and retirement pensions for all employees, and to compel the acceptance of other demands will be to bring about the socialization of railroad transportation by means of government ownership and operation. While it is probable that the counsels of the more conservative leaders

will prevail against the adoption of such a policy, it is not easy to forget what transpired in 1919 when the leaders of railway labor sought to have the government purchase the railroads and make permanent the policy of government operation that had been adopted to facilitate the prosecution of the war.

It may be noted in passing that within a few years after their unsuccessful attempt to persuade Congress to adopt the Plumb plan of public ownership and operation of the railroads the leaders of railroad labor were glad they had been defeated. The prosperity that came alike to railroad management and to railroad labor made private ownership and operation popular. If the current financially sorrowful years are followed by prosperity for industry and transportation—as most of us hope and believe will take place—there will doubtless be no serious effort made by railroad labor to bring about public ownership and operation. However, should prosperity refuse to return in good measure, or should it be very laggard in coming, government ownership and operation of railroads may easily become a live issue.

Legislation for the further regulation of transportation agencies and services is due, indeed it is overdue, but that legislation will be concerned almost entirely with carriers now but partially and inadequately regulated, especially by the Federal government—carriers by water, highway and air—and not with the railroads which during the past half century have been brought under comprehensive regulation, both by the several states and by the Federal government. The Emergency Transportation Act of June 16, 1933, has simplified railroad valuation and has substituted for the former rule of rate-making a simpler and more flexible one that does not impose upon the Interstate Commerce Commission the impracticable and unwise mandate of fixing rates with reference to the value of the carriers' property instead of with regard to the economic conditions that determine the amount that can be and should be paid for transportation.

The same Act has also wisely brought railroad holding

companies under the regulatory jurisdiction of the Interstate Commerce Commission, thus giving the Commission authority over the financing of railroad consolidations and over the financial control by holding companies of such consolidations when formed. Some additional amendments to the Interstate Commerce Act of minor importance are being considered concerning which there is apparently but little difference of opinion. The Coördinator of Transportation has announced that he will make a report with recommendations concerning amendments to existing railway labor laws. The necessity for additional legislation on that subject is not apparent, but it will be well to await Mr. Eastman's report and recommendations before expressing an opinion upon the subject.

THE ARGUMENTS FOR GOVERNMENT OWNERSHIP AND OPERATION OF RAILROADS

What do those who regard government ownership and operation of the railroads in the United States as theoretically wise and ultimately desirable expect will be accomplished by the adoption of that policy? Mr. Eastman has summarized in a few words the answer to this question by stating that "Public ownership and operation would clearly go further than any other change to abate the railroad ills. . . . Public credit would take the place of crippled private credit. Management and operation of the industry would be wholly united. Public regulation would largely merge with management and operation. Financial domination would cease."

The specific "railway ills" referred to in this quoted statement are the extravagant expenditures of capital mainly due to the competition of private railways—expenditures for the construction of branch lines and extensions, costly passenger stations, hotels, produce terminals and warehouses. It is argued that these extravagances would end with government ownership (although just why they should is not very clear), that financial, managerial, and operating benefits would result, and that the ill consequences feared by the opponents of government ownership can be foreseen and forestalled.

The contention that ill effects can be avoided places the advocates of government ownership and operation upon the defensive at the outset, and this explains why the dispassionate and well-reasoned argument of the Coordinator of Transportation in his report of last January upon the Regulation of Railroads so largely resembles the pleas in confession and avoidance to which lawyers are at times wont to resort. The argument, quite naturally, starts with a reference to the fact that most countries of the world have public ownership and operation of railroads, this policy having been adopted "not as a matter of principle but for reasons of expediency." some countries having been forced into it because private enterprise would not build, or could not carry on. Sometimes military considerations have been paramount, or an unwillingness to rely on foreign capital, or a desire to use the railways for the benefit of the general business and industry of the country in its competition with other countries." The fact that few governments have operated their railroads with profit or as self-supporting enterprises is considered to be unimportant. There being a "variety of underlying motives, it is idle to measure the results (of public ownership and operation) by the test of earnings." Nor is a demonstration of evil results of government ownership convincing, because an impressive array of the evils of private ownership of railroads in the United States may be made.

It is further argued that conclusions against public ownership and operation of railroads are not to be drawn from the experience of the United States government in operating railroads during the World War, nor from the trials of Canada as the owner and operator of the Canadian National Railway System. Federal operation of the railroads in the United States was a war measure that accomplished its purpose, quotation being made by Mr. Eastman of the accurate statement of the late Walker D. Hines that "The war needs for transportation were met and railroad security holders were protected from a large part, though by no means all, of the injurious consequences of the War." Ownership and opera-

tion of the Canadian National Railway System was forced upon Canada in 1917, says Mr. Eastman, because "much of this mileage had been recklessly and extravagantly constructed in advance of the country's needs," and "the recommendations of the royal commission of 1917 for divorcement from political control were not followed."

The Coördinator of Transportation optimistically believes it would be possible to divorce political patronage from the railway personnel and service and he thinks the influence of organized labor would not be controlling. He has some apprehension that there is the danger of congressional log-rolling to bring about rate adjustments or construction programs to favor certain groups or localities or to promote means of social relief. To safeguard against such political favoritism he would create an advisory council made up of members selected by business and other groups in the community and give the council authority to confer with and secure information from the trustees of the Corporation by which the government operates the railroads. The Interstate Commerce Commission would also be retained with continued jurisdiction over the issue of certificates of public convenience and necessity authorizing new construction and "also (over) individual rates or groups of rates." The Coördinator realizes that Congress could act directly in fixing rates and ordering construction, but, as Congress has thus far functioned through the Interstate Commerce Commission in rate-making, he thinks it would act likewise under government ownership. It is interesting to note that there is nothing said as to the possible effect upon administrative efficiency of the suggested division of authority between two autonomous bodies having jurisdiction over matters of common interest.

The intellectual candor of the Coördinator is very clearly shown by his doubts as to the possibility of managing the railroads of the United States effectively and efficiently as a single unit. "The railroad industry," he says, "presents very difficult problems of administration." He also states that "If public ownership and operation were established

overnight . . . a machine working smoothly at the outset could not reasonably be expected. . . . It cannot yet be said with certainty that the answer would ultimately be found." The Coördinator is, however, not really so dubious as to the success of government ownership and operation as this statement would suggest. Believing that large benefits and economies would come from the elimination of competition, and in other ways, and also believing that the problems of labor relations would be no greater under government than under private ownership, he would favor the eventual adoption of public ownership and operation of railroads in the United States, although he does not urge present action.

The reasons why the Coördinator does not favor present action are that "There is no aggressive sentiment in favor of public ownership and operation"; that government acquisition of the railroads during the present business depression might increase their fixed charges; and that "owing to the need of dealing fairly with labor, and bringing the new organization into smoothly running operation, the immediate burden upon the public finances might be great." In a word it would be well to wait for better times and until the traffic conditions of the railroads have improved, their competitors have been more adequately regulated, and the strain now being put upon the finances of the Federal government has been lessened. Few will question the wisdom of this suggestion.

THE ARGUMENTS AGAINST GOVERNMENT OWNERSHIP AND OPERATION

The foregoing is a brief, but I trust correct summary, of the best argument I have read in support of the policy of government ownership and operation of railroads in the United States, the policy to be adopted when normal economic conditions return. With at least one of the premises of the Coordinator's argument there will be agreement on the part of those who are convinced that private ownership and operation of American railroads is preferable and should be continued. The conditions that brought about the nationaliza-

tion of railroads in other countries do not prevail or are present in but slight measure in the United States. As I have said elsewhere,¹ "Government ownership and operation of railroads in any particular country is not a question of fundamental principle but of practical wisdom and expediency." As I shall presently endeavor to show, the policy of public ownership and operation of the railroads is not one that the United States *must* adopt. Successful private ownership and management presents no insuperable difficulties. It is a question of what policy *should* be adopted.

It is well to keep in mind the magnitude of the administrative task that the government would assume if it undertook to finance, operate, maintain and develop nearly a third of all the railway mileage in the entire world. Assuming ideal conditions of government administration with the railroads in charge of an executive body free from hampering interference by Congress regarding finances, operating methods, new construction, rate making, wages and working conditions, would a high degree of efficiency and economy be attained? Possibly it might be, but government management in this country has not yet shown itself to be superior to corporate management in initiative and executive ability; and it would seem that the economical management, the efficient technical development, and the maintenance of the requisite esprit de corps of our larger railroad companies fully taxes the administrative and executive capacity of their directorates and officials. Grand consolidation schemes that would bring all the railroads into one system or into five to seven systems have not made much appeal to the public, one of the reasons being a genuine doubt as to the possibility of an efficient, non-wasteful, and technically progressive administration of such huge enterprises. It is claimed by the proponents of public ownership and operation that the unification of the railroads into a single system, the consequent elimination of the com-

¹ *The Annals of the American Academy of Political and Social Science*, Vol. 171, pp. 180-184, Jan. 1934. This article states concisely the arguments for private ownership and operation of railroads in the United States. In the present paper I have drawn upon the argument presented in that article.

petitive practices of rival corporations, the substitution of government for corporate financing, and giving railway laborers the status of government employes, will so simplify the problem of administration as to enable the government to solve the problem fully and easily. While there is no mathematical formula by which this claim can be proven sound or invalid, there is strong circumstantial evidence that its advocates are unduly sanguine.

In the first place is it certain, indeed is it probable, that the executive authority charged with the management of the railroads in the United States would be free from Congressional or political interference with administration, with the adoption and carrying out of policies as regards wages, working conditions, rates charged, and services rendered? Organizations representing the agricultural and manufacturing interests, labor, silver mining, ex-soldiers and other groups and blocs are certainly not without influence at present upon congressional and administrative policy and action. Does our past experience warrant us in assuming that there would be a non-political administration of the railroads if owned and operated by the United States Government?

It is not to be expected that government management would reduce the costs of operation. Operating expenses are determined mainly by two factors, operating and administrative efficiency and labor costs. Nearly two thirds of the railway operating expenses—63.52 per cent in 1932—are for wages, which in that year amounted to 48.3 per cent of the gross operating revenue. In 1920, the year that the war-time operation of the railroads by the government ended, labor costs were 59.5 per cent of operating income and were 63.2 per cent of operating expenses. The substitution of government for corporate ownership of the railroads would not reduce the number of employes. The number would quite certainly be increased, and is it not equally probable that the hours of labor would tend to be lower and the wages paid higher under government management, even if hours of labor and wages were not influenced by political forces, which is an assumption that is very difficult to make?

Would the rates paid by the travelling and shipping public be lower under public ownership and operation of the railroads? Railway revenues are not now large, indeed not so large as they need to be and as it is hoped they will be, when prosperous times return. These revenues are derived from fares and rates fully regulated by the government. If the government in managing the railroads should charge lower rates than private management, subject to public regulation, it would have to reduce operating expenses and capital charges substantially or else incur an annual deficit to be borne by the tax-paying public.

There will doubtless be general acceptance of the proposition that the government should operate the railroads on a self-supporting basis; that it should obtain revenues sufficient to cover fixed or capital charges, as well as operating and maintenance expenses. The tendency of the government in fixing charges for its services is to consider only operating expenses and to ignore capital costs, but if the government should incur an indebtedness of \$25,000,000,000 or even \$20,000,000,000 to acquire the railroads, either the railway revenues or the taxpayers would have to carry annual interest charges of some \$750,000,000. There is, moreover, the question whether the local, state and Federal governments shall forego or shall continue to receive the taxes now being paid by railroad corporations, and, which before the current business depression amounted to more than a million dollars a day. Presumably the users of the railroads should continue to pay the taxes instead of their being shifted to the general public.

If, then, as seems probable, railroad operating expenses would not be reduced by public management, and if revenues are to be obtained sufficient to meet those expenses including maintenance charges, to provide funds covering not only obsolescence but outlays for such new kinds of equipment as must be made to keep the service abreast of technical requirements, to pay taxes equal to those that would be levied upon corporate owners, to cover interest upon the government's

investment in railroads, and possibly to amortize the government's investment over a period of fifty years—as it is now proposed to require private railroad corporations to provide from earnings for the amortization of their bonds—if this is the policy followed in the management of the railroads by the government of the United States, it is difficult to see how fares and rates could be lower than would be charged by corporately owned railroads whose services, charges and finances are subject to comprehensive government regulation.

It is generally assumed that the cost of carrying the capital now invested in railroads and of securing such additional funds as may be required in the future will be reduced by nationalizing the railroads. The government can ordinarily borrow money at lower interest rates than even the best corporations are obliged to pay; but will this surely be so when business again becomes active? It will be well to keep in mind that the recent World War left us with a large debt, that the large sums owed us by foreign countries are apparently to be paid only in small part, that our large national debt of 1932 will be increased fifty per cent by the end of 1935 when it will reach or exceed \$32,000,000,000, with a prospect of growing larger instead of smaller. To add twenty to twenty-five billion dollars of railroad bonds to the thirty-five billion dollars of other indebtedness that the United States will doubtless be carrying five years hence will make it uncertain whether the Federal government can command capital at lower rates of interest than will need to be paid by well-managed corporations. If, as I do not expect, the government should fall into the financial morass of monetary inflation and consequent debt repudiation, it would be without credit or borrowing power.

PRESENT AND FUTURE FINANCING OF PRIVATE RAILROADS

The Coördinator of Transportation is correct in stating that if government ownership and operation of railroads comes about in the United States it will be because "private enterprise and capital will not be able to carry on successfully."

Thus private versus government ownership in the United States in the future is primarily and principally a question of finance. Is the present financial condition of American railroads and are their future prospects such that private capital, adequate in amount and at a reasonable cost, can be obtained to provide for their maintenance and development? Obviously it will not suffice to answer this question categorically with a simple yes or no. The financial prospect of American railroads is conditioned upon a number of determinants, upon their present obligations and those of the near future; upon the factors that control or affect their gross earnings—industrial activity and the conditions under which the railroads and their highway and waterway rivals are to compete for the consequent traffic; upon the factors that affect net earnings, *i.e.* efficiency and economy of corporate operation; and upon the expenses that may be added by legislation affecting taxes, labor costs, and operating requirements.

Moreover, the future financing of American railroads, as well as other business activities and undertakings, will be affected by the policy developed by the government towards private enterprise in general and as regards legislation regimenting production and socializing the results of productive effort. This paper cannot hope to present more than a summary diagnosis of the railroad financial situation and a similar prognosis of the recovery of the railroads from their present illness. Moreover, it will be necessary, and this is doubtless the reader's good fortune, to forego consideration of social and political factors, and to confine the discussion to the present and near-at-hand financial obligations of the railroads in the United States and to the prospect of their being able to finance those obligations from private sources with such temporary assistance as the government is now giving and is manifestly willing to afford.

The latest and best diagnosis of railroad finances that I have seen is that contained in the "Report of the Transportation Conference of 1933-4," published under date of March 16, 1934. This conference was organized in the summer of

1933 upon the initiative of Mr. Harry A. Wheeler, the President of the Railway Business Association, and was composed of representatives of fifteen national financial, business, and transportation associations. The Conference had nine meetings of two days each over a period of about eight months. Its secretary was Dr. Lewis C. Sorrell, Professor of Transportation of the University of Chicago, who was assisted by a staff of investigators and who, presumably, drafted a report for consideration by the Conference. Section I of the report deals with government versus private ownership of transportation facilities and contains an instructive analysis of the present and prospective status of the finances of American railroads.

There is plenty of pigment ready at hand that might be used in painting a dark-toned picture of the present railroad landscape. Such a picture is presented in the discussion of "Railroad Ills" in the January 1934 report of the Coördinator of Transportation who states that

"Viewed from the standpoint of average or aggregate railroad conditions, the future credit outlook seems most unpromising. . . . The practical question relates to the credits of the individual railroad companies. Few of them meet the standards of private investment now, but some will measure up with improving business conditions. Many others will find it a very slow and difficult process to meet those standards, even if traffic conditions greatly improve; and consideration of average conditions suggests that the number of delinquents will exceed the number that are able to qualify."

The condition and prospects of the railroads may not be so discouraging as this statement indicates; but we shall be more successful in solving the financial problems confronting the railroads in the United States, if we fully appreciate and do not underestimate the difficulties to be overcome. We know that the railroads and other business enterprises will soon be seeking capital in large volume while the government is at the same time reducing the available supply by increased taxes and by new loans made necessary by greatly augmented

public expenditures. It is possible, but I do not think probable, that the Securities Act in its present or amended form may hamper the railroads in selling their securities. The traffic and earnings of the railroads are at a low ebb, and, although the tide has begun to flow in, there is no hope that it will reach the height of the flood tides of 1926 or 1929. Indeed, the height attained by railroad traffic and earnings will depend in no small measure upon legislation yet to be enacted establishing equality of competitive conditions among the several agencies and facilities of transportation, the waterways, highways, and railroads. Moreover, the receipt by capital of a fair return from the net income of the railroads may be, but I do not think it will be, made impossible by legislation largely increasing labor costs. This incomplete recital of the difficulties to be overcome does, indeed, give a gloomy aspect to the foreground of the picture; but I believe it is possible, without distortion of the perspective, to lighten the background of the railroad financial landscape by introducing some brighter colors.

The broad facts as to the present value of the assets of the railroad companies and as to railroad capitalization have been determined and presented by the Bureau of Valuation of the Interstate Commerce Commission, which estimates that the original cost of railroad carrier property, as of December 31, 1932 (not including the non-carrier property owned by the railroads nor any intangible elements of value), plus land values as of June 1, 1933, and working capital, was \$26,232,000,000. After making reductions in this estimate to allow for the cost of land being less than the appraised value June 1, 1933, the total becomes \$24,000,000,000. The Commission further estimates that the cost of reproduction new on the basis of the low prices of June 1, 1933, less depreciation in the property other than land, plus the 1933 value of the land, plus working capital, would be about twenty-one billion dollars—\$20,971,000,000.

The capitalization of the property thus valued, the securities outstanding December 21, 1932, included

Stock.	\$10,226,070,233	
Funded debt.	13,347,486,355	
	<hr/>	
Total.		\$23,573,556,588

By omitting intercorporate holdings of these securities, the amount of railroad stocks and bonds in the hands of the public becomes

Stock	\$ 7,150,374,952	
Bonds.	12,338,687,304	
	<hr/>	
Total		\$19,489,062,256

It will be seen from these totals that the outstanding railroad capital in the hands of the public was four and a half billion dollars less than \$24,000,000,000, the estimated original cost of existing railroad property reduced by making allowance for the cost of land being less than the appraised value June 1, 1933; and it will be noted also that the estimated cost of reproducing railroad property under the prices obtaining in June 1933, less accrued property depreciation, plus the value of the land in 1933, and the working capital—\$20,971,000,000—was nearly a billion and a half dollars in excess of the total securities, stocks and bonds, in the hands of the public.

The ratio of bonds to total capitalization, 56.6 per cent, at the end of 1932, or 62.3 per cent when intercorporate holdings of securities are eliminated, seems undesirably high, but the ratio of bonds to stocks was the same in 1920. However, as Mr. Eastman points out in his report upon Regulation of Railroads—

“The funded-debt situation [of American railroads] is in certain important respects better now than in 1920. Then the total outstanding capitalization amounted to 101 per cent of the book investment in road and equipment and the funded debt amounted to 56.7 per cent of that investment. In 1932, the corresponding percentages were 86 and 49. This improvement was due to the increase in corporate surplus.”

The ratio of funded debt to the total capitalization of American railroads in 1920, and also the ratio of funded debt

to total book investment, would have been lower had government regulation of the rates and revenues of the railroad during the decade preceding the entry of the United States into the World War not kept the net income of the railroads at a level that discouraged the flotation of stocks and encouraged the issue of bonds to secure new capital or to pay off maturing debts. The present ratio of bonds to total railroad capitalization is in no sense a cause for alarm or special apprehension, but it may well be the policy of the Interstate Commerce Commission, in its future regulation of the issue of railroad securities, to bring about a decrease in the ratio of bonds and an increase in the ratio of stocks, to total capitalization. This gradual reduction in the burden of fixed charges will give greater stability to the structure of railroad finance.

The finances of our railroads as a whole at the present moment, at least on the surface, don't look very bright. They have a dark blue shade. During 1932, their total operating and non-operating income was only 78 per cent of their fixed charges, and in 1933 the income was not quite equal to the fixed charges, being 98 per cent. This, of course, is a measure of the effect of the current business depression, which effect has been augmented by the desperate struggle of but very partially regulated competitors of the railroads for traffic at rates that would enable them to survive. Fortunately for the railroads, traffic and earnings have begun to improve; 1934 will be better than 1933 and 1932 were. It may take 1935 and 1936 to get back to the 1930 level. The railroads will not reach the heights attained in 1929, but that is not necessary. During the five years ending in 1930, American railroads had total operating and non-operating income amounting to 2.04 times their fixed charges, although in 1930 the investment in property, less accrued depreciation (\$23,700,000,000), was 54 per cent greater than 1911, when the total net income was 1.77 times the fixed charges. A glance backward thus gives us encouragement for the future.

Assuming that the net income of American railroads as a whole will enable them to meet their current fixed charges,

what about their short time loans that have been increasing so largely of late, and their equipment trust notes, and their funded debts that will mature this year and next year, and during the following five years? What are these debts, and can they be taken care of successfully? The facts as to the amounts and the nature of these debts are summarized in the Report of the Transportation Conference of 1933-4 in the following table which states the sources from which the information presented was obtained:

THE TOTAL FUNDED AND UNFUNDED PAYABLES OF CLASS I RAILWAYS,
1934 TO 1940, INCL.

(a) Short time loans and bills payable Dec. 31, 1933 (1)	\$ 337,909,946
(b) Borrowings from the Reconstruction Finance Corporation, net Jan. 31, 1934 (2)	340,726,000
(c) Funded indebtedness maturing 1934-5 (3)	456,189,189
(d) Funded indebtedness maturing 1936-40 (3)	961,319,589
(e) Equipment trust notes, 1934-1940 (4)	518,715,077
Total	\$2,614,859,801

Sources:

1. Bureau of Railway Economics.
2. Reconstruction Finance Corporation, Public release Feb. 9, 1934.
3. Computation Conference research staff from Moody's 1933 Manual.
4. Computation Conference research staff.

The table summarizes the debt problem confronting American railroads as a whole. To understand whether and in what measure the problem can be solved it is necessary to analyze the finances of each of the railroad companies, at least of the 156 companies operating Class I railroads (those whose annual gross earnings are \$1,000,000 or more), and thus to "estimate the credit position of the individual roads and the probable amount of government assistance they may need in order to avoid possible receiverships." This estimate will indicate whether the government advances will be so large as to make probable public ownership and operation. Those who prepared the Report of the Transportation Conference of 1933-4 made such an analysis by considering for each of the Class I railroads the following factors:

1. Fixed charges on rentals for lease of road.
2. Interest on funded and unfunded debt.
3. Bond maturities.

4. Equipment trust maturities.
5. Reconstruction Finance Corporation loans.
6. Loans and Bills Payable.

Information having been compiled as to these several factors for each of the Class I railroads, the companies were classified according to their financial strength or weakness into seven numbered groups, the subdivision of the roads in the first two groups into "a" and "b" categories making in fact nine groups.

The roads in group 1a (mileage 43,335) have had net earnings, throughout the depression, more than 1.5 times their fixed charges, "or have not fallen below that figure more than a single year."

The carriers in group 1b (mileage 17,396) are not quite so strong as those in the first group but their bond maturities come late in the seven year period ending with 1940 while their loans from the Reconstruction Finance Corporation, their other loans, and their bills payable are small, and their fixed charges have been more than earned each year.

The railroads in group 2a (mileage 26,432) have not quite met fixed charges each year of the depression, but their bond maturities are small or have been deferred. The companies have time to recover and they have but small Reconstruction Finance Corporation or other loans, and their bills payable are also light. A moderate improvement in business will take care of these companies and they will need no government assistance.

In group 2b (mileage 40,048) are companies whose "bond maturities are perhaps somewhat more troublesome," and their Reconstruction Finance Corporation and other loans and their bills payable are "substantial"; but a moderate enlargement of business will enable them to meet fixed charges. They may need to have an extension of their present loans from the government, and may require some "government assistance in funding floating loans and bills payable" or refunding maturing obligations. However, but moderate government aid will be necessary.

The railroads in group 3 (mileage 27,761) can meet their fixed charges with a moderate increase in traffic, but their maturing obligations and also their Reconstruction Finance Corporation and other loans and their bills payable are heavy. These companies "are likely to need substantial assistance in 1934-5; possibly also in 1936-40, unless business recovers in a marked degree."

The companies comprising group 4 (mileage 27,857) are "on the edge." To enable them to meet fixed charges there must be a marked improvement in business. "There are some near maturities of substantial amounts; also heavy Reconstruction Finance Corporation loans, and loans and bills payable. They will need substantial aid to avoid difficulties." Here is where government assistance is most definitely needed and where it will be most helpful.

Group 5 contains the railroads now in receivership (mileage 41,903 miles). Presumably many if not most of these companies will need to be so reorganized financially as to bring fixed charges definitely below future net earnings. Further government aid does not enter into the present calculation, although it may be possible that the government may later be of assistance to the reorganized companies in strengthening their financial structure. It is those who have invested in the securities of the companies in this group that will suffer largest losses from the effects of the business depression upon the railroads. Similar losses have been incurred in previous depressions, and are now being borne by investors in many kinds of business.

In group 6 are the Canadian roads in the United States (mileage 5,971). They have their financial problems to the solution of which Canada is giving attention.

Group 7 (mileage 9,575) comprises "miscellaneous Class I roads, usually of small mileage, and generally identified with other roads or interests."

Such is a summary of an analysis of the financial status of the Class I railroads in the United States. How much government assistance will be required, if private ownership is con-

tinued, and will private capital sufficient to meet the future needs of the railroads be available and forthcoming? The answer here made to this difficult question will be a summary statement of the one made in the Report of the Transportation Conference of 1933-4.

The answer given in the Report is necessarily based upon several assumptions that are deemed to be warranted by the facts of the present situation and by a conservative estimate of the tide of events in the near future. One assumption that will hardly be questioned is that business conditions in 1934 and in 1935 will be as favorable to the railroads as they were in 1933, "and are likely to be somewhat better." That will mean that the railroads in groups 1*a*, 1*b* and 2*a* will not need government aid. The railroad companies in group 5 that cannot otherwise become solvent will be undergoing financial reorganization with the inevitable losses to investors that are incident thereto.

It is assumed that with such improvement in business conditions as may be expected, the railroads can be given, and will receive, a fair share of bank credit to assist in carrying loans and bills payable; and "that in those cases where the volume of such loans and bills outstanding is large, and some relief to the banks is necessary, a reasonable portion of that loan may properly be transferred to the government." Other assumptions are that the Reconstruction Finance Corporation will extend the maturity of loans to railroads when that is necessary; and that, when some government aid seems to be needed in refunding maturing debts, the government will assist by assuming a reasonable part thereof, as was recently done for the New York Central Railroad. As regards railroads in groups 2*b*, 3 and 4, whose earnings may not be sufficient to meet fixed charges, it is believed that deficits will not extend beyond 1934 and 1935. Likewise it is assumed that there will be no need for assistance after 1935 in meeting equipment trust maturities. As concerns possible legislation, the reasonable assumption is made that Congress will not now pass laws reducing the standard day in railroad service

from eight hours to six hours without reduction in pay, limiting the length of trains, or compelling the railroads to adopt a comprehensive employee pension system.

If those several assumptions be accepted as valid, what will be the probable amount of government assistance that the railroads in the United States will need during 1934 and 1935 and during the succeeding five years? The estimate made in the Report of The Transportation Conference of 1933-4 is that the railroads in groups 2b, 3 and 4 will need government loans amounting to \$195,000,000 during 1934 and 1935, and \$72,000,000 during the following five years, a total of \$267,000,000, it being understood that some assistance may also be given to group 5 railroads when and as financially reorganized. This, of course, can be only an estimate, but it is one based upon a careful study of the financial condition of each of the Class I railroads.

The present and prospective loans of the government to the railroads include the following sums: Loans made by the Reconstruction Finance Corporation up to the end of January 1934, \$340,000,000; loans authorized by the Public Works Administration for improving railroad facilities and increasing employment, \$200,000,000; estimated additional government loans up to 1940, \$267,000,000. The total of these three sums amounts to \$807,000,000. Should the Public Works Administration advance another \$200,000,000 for purposes similar to those for which it has made loans, the total government advances would not much exceed \$1,000,000,000. That total of possible government advances may very properly be compared with the \$975,000,000 that the government loaned to the railroads during the World War and the few years thereafter. The railroads have paid off over 95 per cent of those loans and have paid interest thereon at rates higher than the government paid to secure the funds to make the loans. We can hardly hope that conditions will be as favorable for the payment of debts during the coming ten years as they were during the decade that ended with 1930, but even so one does not need to be over optimistic to think that American rail-

roads will be able to liquidate a debt of a billion dollars to the government during the next ten or fifteen years.

American railroads will, during the next few years, need to obtain from private investors much more capital than will be required to pay off the government loans and to liquidate or refund maturing obligations. There is a large accumulation of deferred maintenance, which means that more than a normal share of the earnings from operation must, for a period, be currently spent upon track, other structures and equipment. A conservative estimate has placed present accrued depreciation at \$600,000,000. The amount is probably greater than that; and, obviously, as business revives, and consequently railroad traffic and revenues increase, many railroad companies will have to devote to maintenance funds that would otherwise be distributed in dividends to stockholders. If the revival of business is slow the overcoming of deferred maintenance will constitute a serious problem; while if business recovery should be relatively prompt and definite, as we now have reason to hope it will be, the railroads will in a few years have their properties in normal condition.

One other need of the railroads for additional capital may be difficult to meet. American railroads have had an enviable record, especially during the past decade, in improving their facilities and services. Betterments always cost money; and, although they sometimes so reduce expenses as to make the additional investment self-liquidating, that by no means applies to all expenditures for improvements. Even at the present time, partly with the aid of government loans, betterments are being made in passenger and freight facilities.

Obviously, more could and would be spent if business conditions were better; and, while the railroads may be expected to do their best in introducing improvements during the next few years, they will have to follow a conservative policy. During the prosperous years preceding the current depression American railroads as a whole spent about \$700,000,000 annually upon improvement and betterments. Such large outlays will not be possible in the near future; but the

public may confidently expect railroad expenditures for the betterment of facilities and services to increase at least *pari passu* with the growth in traffic and revenues. The current business depression has inevitably slowed down the technical progress of the railroads but only temporarily, not permanently.

GENERAL CONCLUSIONS

What conclusions may be drawn from this general survey of the financial condition and problems of American railroads? Will they be able to secure necessary capital from private sources? Do those of us who would prefer private ownership and operation of the railroads, and we comprise the great majority of the people of the United States, need to feel apprehensive?

Successful private financing of any business enterprise depends upon the confidence of the public in the enterprise. It is a matter of public credit, and that, particularly in the case of the railroads, may be largely determined by the attitude or policy of the government. There is no evidence that it is or will be the purpose of Congress or of the administrative authorities of the government, either by the enactment of new laws or in applying existing statutes, to pave the way for the nationalization of the railroads. It might be helpful to the railroads, and strengthen public credit in them, if the government were to make a clear-cut statement that the loans it is making are made to strengthen the position of the railroads as privately-financed enterprises subject to government regulation; that the government does not favor public ownership and operation of the railroads; that in the regulation of rates and services it will be the policy of the government, while safeguarding the interests of the public, to allow the railroads such rates and revenues as economic conditions warrant; and that the railroads are to be regulated as economic enterprises and not as agencies to be employed for the realization of social aims other than the aim of obtaining progressively efficient and economical transportation.

The future strength of railroad credit will depend in part

upon the action Congress may take upon pending proposals which if enacted into law would largely increase labor costs. Such an increase would make impossible the operation of the railroads upon a profitable, or, in the case of many railroads, even on a self-sustaining, basis for the present or in the near future. I have no doubt that this fact is known by Congress and will determine its action.

Congress also has another patent responsibility regarding railroad credit. It has before it the Rayburn Bill for the regulation of interstate motor carriers, a bill that has been evolved from previous bills, from lengthy hearings and much counsel. Moreover the two bills prepared by the Coördinator of Transportation, one for the regulation of interstate motor carriers and the other applying to carriers engaged in interstate transportation by water, have been introduced. In sending to the President for transmission to Congress the bills drafted by Mr. Eastman for the regulation of interstate motor and water carriers, the Interstate Commerce Commission stated, "In our judgment the enactment into law of the . . . two bills is imperatively necessary under present conditions." Thus the Commission is in full agreement with the Coördinator when he states in his Report that "The conclusion is reached that the entire transportation industry, including the other agencies as well as the railroads, is in need of the guiding hand of government control," government regulation being the measure of control recommended. There is no doubt that railroad credit, and I believe the credit of motor and water carriers as well, would be, and I hope soon may be, strengthened by applying to the several agencies of transportation (as is proposed by the Coördinator and the Commission) the same general principles of government regulation, with such differences in the provisions of the statutes as the special characteristics of the several agencies regulated may necessitate.

There are those who seem to be apprehensive about the general monetary and fiscal policy that Congress may adopt or authorize. Uncontrolled or excessive inflation of the

currency, accompanied by largely increased expenditures and taxes to correspond, would, of course, make impossible the continuance of private ownership and operation of the railroads; but I don't think we need to assume that the American people are going to commit economic hara-kiri.

In general, the conclusion seems warranted that there is nothing in the present railroad situation that makes private ownership and operation impossible. Railroad expenses have been greatly reduced per unit of service rendered and can be kept below former levels, unless that should be prevented by legislation. The railroads are being operated with increasing economy and efficiency. Their gross and net earnings, while still very low, are on the upward curve, and will rise with the recovery of business. The people of the United States can have private ownership and operation of the railroads, if they so wish, and as long as they wish. Government ownership and operation is neither necessary nor desirable, and should be avoided.

SOME SUBJECTIVE FACTORS AFFECTING INDUSTRIAL RECOVERY

SAMUEL PRICE WETHERILL

(Read April 21, 1934)

AN impressive list of writers within the last decade have called attention to the shortcomings in the aims, organization and methods of modern civilization. Many of their writings antedated the industrial depression. Some predicted it, and cited psychological causes which in their judgment could not fail to produce a serious depression.

We all hope for an immediate return of full prosperity, but the far-sighted prefer a slower recovery grounded in improved fundamental factors and avoiding the perpetuation of those economic and social ills which were revealed by the depression. In other words, we want prosperity but we do not want its attendant ills.

The American Philosophical Society, with its periodical reviews of the scientific marvels reflecting the potency of the human mind, is admirably situated fairly and wisely to appraise our predicament, its scope and causes.

Although each specific field of effort is immersed in its own problem, we of this Society are free to take a detached and over-all view, to see the wider human implications of the common problem of recovery. The very fact that this symposium is scheduled acknowledges our sense of responsibility that scientifically minded people should seriously address themselves, not alone to the interesting investigations of pure science, but to the practical human need for profound thinking in search of a permanent cure for industrial and consequent social ills.

In our detached perspective, then, we may fairly ask whether the responsibility lies wholly or essentially with any one aspect of our collective life. Shall we blame the govern-

mental, the religious, the scientific and educational, the social, the economic, the productive aspects of our collective structure, or shall we admit that the responsibility may fairly be laid in part at the door of each? Would it be the part of wisdom to allow any one group to dictate its special conception of a proper solution to our common ills?

What if, underlying the economic and all the other aspects of the problem, we were to find a cracked and weakened "philosophy" of life—obsolete and inadequate to our modern age?

Let us at least probe to the roots of the disease in the hope of diagnosing it correctly. This much is worth while, even if we fail to prescribe a cure. Let us view the problem from the philosophical rather than the specialized perspective in the hope of being helpful at a time when help is sorely needed.

Beneath the obnoxious symptoms of industrial illness, we must assume that there lies a hidden disorder in the relation of cause to effect. Notwithstanding innumerable expert diagnoses which trace the effects to details of economic technique and procedure, there is a very responsible and formulated public opinion to the effect that the causes go deeper, and are themselves to be found in the realm of subjective motivation, however objective their symptoms.

The writer leans strongly to this view. Most of us can sense a maladjustment in the evident conflict between private and public obligations which are imposed upon us by contemporary conventions. Society seems to compel a choice of vocations—the one involving concentrated dedication to a program of self-preservation almost regardless of its consequences upon the common good, the other involving sacrifice of legitimate self-interest in complete dedication to the collective welfare.

We have corporations frankly organized for profit, which we charter with privileges but loosely attuned to the public interest. These corporations dedicated solely to the profit motive often see dividend opportunity in programs and policies subversive to the common good. On the other hand,

we have non-profit corporations, which compel an order of selflessness which is almost prohibitive to those of modest income. It would seem that there is now a real opportunity for us to evolve an intermediate form of charter somewhat like the limited dividend corporation, but designed to encourage constructive social effort and still permit of satisfying rewards commensurate with the abilities required for constructive achievement.

In America, men of marked business ability devote the most creative years of their lives to the accumulation of wealth,—and later try to retire or undertake constructive civic work.

Would it not be more satisfactory to the individual and more beneficial to society if in their more vigorous and adaptable years they sought wealth and civic betterment simultaneously?

Thus there would be fewer of those antisocial corporate practices which do much to nullify the subsequent philanthropies of former corporate heads.

It may be said that such a duality of interest would defeat the profit motive,—doubtless to some extent it would under present corporate practices,—but inasmuch as most experts recognize the intimate interdependence between the prosperity of the consumer and that of the producer, it would appear to me that coöperation and sympathy with the public toward the common objectives would create confidence and sensitiveness to market requirements and improve the profit opportunity. At the same time, the need for governmental interference with business should be greatly reduced. This would tend to reduce taxes, and increase profits without the exploitation of public interest which now causes so much expense and confusion.

I cannot here set forth in detail the kind of charter which would mutualize public and private interest and at the same time preserve a reasonable profit incentive for men, management and money. It should, however, insist upon public benefit, whether in the form of rebates to consumers, as in

the case of mutual insurance policyholders, or in the form of direct dedication of surplus profits to non-profit public enterprises.

Some political friends perhaps might prefer the system of confiscation of earnings—named taxation—which is so rapidly engulfing us, with a mounting cost of government tending to compel what might better be done voluntarily and in a more expert and effective manner.

A practical start in the direction of mutualizing utilities as certain life insurance companies are mutualized, might wisely be sought by those who see a danger of government ownership and operation.

Pending this ideal, let those who have the loyalty and the vision cooperate and make what progress can be made.

If the will is strong enough the way will become more obvious as we pursue it.

It seems that society has scarcely graduated from the primitive battle royal psychology, and that it offers its rewards more to the crafty, the brutal and the strong than to the conscientious and the considerate.

True, the simile of the individual battle royal does not hold, since the combatants do not represent themselves alone so much as they represent organized minorities and groups having special interests to advance, too often with a ruthless disregard of the consequences upon society as a whole. I cannot avoid the conviction that history will view the present conditions in the national capitol as a rather disgraceful scramble for private at the expense of public interest. It might be described as a battle royal on an heroic scale, but still expressing the primitive battle royal psychology in the conflict of not only selfish but actually vicious lobbies.

The two great political groups are being forced into alignment, one dedicated to the doctrine that society is paramount and individuals must get in step with the social needs or go to jail. the other to the doctrine that individualism is the backbone of society and to stultify it is to ruin the social order. To some extent, of course, they are both right. Is

it not rather tragic to be forced to choose between the heart and the head of the body politic? One's strong preference would be to preserve both, and to hope for an impulse to a philosophy of life which would correct and harmonize the separate motivations, neither of which can be spared, and both of which are essential to healthy recovery at home and abroad.

We must recognize as evils and avoid such disintegrating disease symptoms as sectionalism, factionalism, doctrinairism; we must resent being forced into classifications as "laborers," "capitalists," "individualists" and "collectivists."

We must insist that we are all actually or potentially workers, capitalists, individualists and collectivists at one and the same time.

There is, I believe, real philosophical and practical merit in this simple suggestion. I believe it goes to the root of our industrial and social ills, and that the rival forms of group exploitation to which we are systematically subjected would have no essential function to perform, at least not in such virulent forms as now beset us, if the common motivation were amended in keeping with the spirit of the above diagnosis.

I believe our country in order to get well, needs a new slogan, a new concept in keeping with the spirit of the new day, but retaining what was good in the old. We need some inclusive symbol which will signalize the end of the spirit of the battle royal, and bring on a richer spirit of mutuality, a nicer sense of relative values, an abandonment of the concept that one faction must succeed by thwarting other factions.

If we must have an "ism," we could rally under the banner of "Mutualism" as the militant expression of the philosophy of mutuality or interdependence.

We could develop an integrating program for spreading this mutually inclusive doctrine, and for realizing this objective which would do away with the need for many of the disintegrating "isms."

We could offer "mutualism" to our friend and writer of one of yesterday's valuable papers, Mr. Frederic A. Delano,

as an over-all civic objective for his group of co-workers in the American Civic Association, and ask that these patriotic citizens, who have already done so much, now formulate a nation-wide campaign of coöperation by all groups in establishing a new and modernized plan based upon this integrating philosophy of American citizenship.

As social engineers, loyal to humanity and to each other, we have only to face the task of regenerating and expanding our conceptions of self-interest and of fitting to them a new technique of organized intelligence and common purpose.

Industry cured of its symptoms could not stay cured unless business and banking which restrain and limit its operations are cured also. Industry of itself is the healthiest organ of the body politic at this moment. Its proper function is maximum production at minimum cost, and it is standing by ready for any and all demands which may be made by a confident buying public upon it. It is capable of producing whatever is needed, whenever it is needed, at prices and in quantities that indicate abundant vigor.

Let industry be relieved of the deterrents imposed by the public's lack of confidence in the social mindedness of the leaders of business, as expressed in hostile and often foolish legislation.

Let the duality of purpose herein recommended find expression in a new form of mutualized corporate effort, and it seems inevitable that confidence will return, the sinister criticisms of our fundamental institutions cease, capital flow into useful channels, governmental extravagances become unnecessary, and private initiative again be stimulated.

Having frankly acknowledged that our ills are due to shortsightedness, let us correct our vision and see our obligation to society in a new perspective.

In closing, permit me to quote from "Business Adrift," by Wallace Donham: "We must have a general plan for American business, and behind that plan a sound social philosophy. . . . Mankind is now in one of its rare moods of shifting its outlook. The mere compulsion of tradition

has lost its force. It is the business of philosophers, students and practical men to re-create and re-enact a vision of the world, conservative and radical, including those elements of reverence and order without which society lapses into riot, a vision penetrated through and through with unflinching rationality.”¹

¹ “Business Adrift,” Wallace B. Donham, McGraw-Hill Book Co., 1931.

PENROSE MEMORIAL LECTURE
A GENERATION'S PROGRESS IN THE STUDY OF
EVOLUTION

EDWIN G. CONKLIN

(Read April 20, 1934)

THE Penrose Memorial Lecture is not intended to be primarily or chiefly a personal memorial of our munificent benefactor. Such a memorial address was given last year by Professor Waldemar Lindgren of the Massachusetts Institute of Technology. It was my good fortune to have known Dr. Penrose for many years and to have been officially associated with him both in this Society and in the Academy of Natural Sciences, but I could add little to Professor Lindgren's admirable address. Dr. Penrose was preëminently a gentleman and a scholar. He was a man of dignified modesty, transparent honesty and sincere devotion to genuine science and learning. The form of memorial which he would have most prized was one making for the promotion of science, rather than for personal praise or vain show. In the language of Professor Lindgren's address of last year, he said in substance in making his bequest: "Here is the money, take it and use it wisely and well. There are many things I would have liked to do, but could not: life was too short. You try to accomplish them! Carry on!"

This the American Philosophical Society is trying to do, chiefly through its grants in aid of research, its publications, library and meetings. Of the grants made during the past year from the income of the Penrose Fund six were in fields in which he was especially interested, while all of them, we believe, are for projects which he would have approved. "He rests from his labors but his works do follow him."

I

If one were searching for the most inclusive subject in modern scientific research what other topic would touch so many fields as that of evolution? In the non-living world it includes almost everything from the evolution of atoms to that of universes; in the living world practically everything from amoeba to man, from germ cells to developed organisms, from reflexes to reason, from savagery to civilization. Almost all the work of modern science and learning could be classified under some of these fields. The small part of this vast theme which I shall touch upon in this address concerns merely some of the recent work on the methods and causes of organic evolution.

Thirty-eight years ago I, a newcomer to Philadelphia, was introduced to the American Philosophical Society as one of the speakers in a symposium on "The Factors of Organic Evolution." I had been urged by Professor Cope and by the then presiding officer of this Society, the late Dr. William Pepper, to take part in the symposium, but being painfully aware of my own inexperience and greatly overawed by the dignity of the Society and the distinction of its members I had begged to be excused. Dr. Pepper encouraged me then, and his words have heartened me many times since, by saying, "You know we *can* do what we *have* to do." But when I still plead my inability to take part Dr. Pepper finally said to me, "Well, Professor Conklin, we had hoped to get acquainted with you." This challenge I could not ignore and at once I said, "Very well, I accept." It was the casual recital of this episode that led President Morris to draft me to speak on this occasion on "A Generation's Progress in the Study of Evolution."

The symposium occurred on the evening of May 1, 1896, the speakers being Professor Edward D. Cope, Professor Liberty H. Bailey of Cornell University, and myself, and since our addresses represented fairly well the methods and conclusions of students of evolution a generation ago, I will briefly

state a few of their principal conclusions. Cope¹ maintained the Lamarckian point of view that variations are the materials of evolution and that they are caused (1) by the direct action of the environment on developed organisms (his Physiogenesis), (2) by the inherited effects of use or disuse (his Kinetogenesis), (3) by the energy of growth forces (his Bathmogenesis) and (4) by sensations or consciousness (his Arch-aesthetism).

On the other side I championed² the Weismannian view that (1) acquired characters are not inherited, (2) that inherited characters must be predetermined, but not preformed, in the germ cells, and in particular in sub-microscopic inheritance units, (3) that all hereditary variations are caused by the action of extrinsic forces on the germinal protoplasm, producing changes in its structure, rather than upon developed organisms, and finally (4) that the only way of breaking the deadlock between Lamarckians and Darwinians was by means of experiment. In the light of subsequent events I think I have no reason to regret my immature contribution to this symposium.

Professor Bailey's³ philosophy was neither strictly Lamarckian nor Darwinian although in general it leaned to the former; it was rather *sui generis* and might be called Bailey-an. He maintained that variability is the original law of organisms, that like no more produces like than unlike, but that mutability is a fundamental and normal law, while heredity or permanency is an acquired character. The organism is shaped by its environment, and nature eliminates the non-variable and favors the survival of the unlike.

This account of a long forgotten program in the history of this Society is useful merely as indicating some of the opinions and speculations regarding the causes of evolution a generation ago. In what follows I must beg the indulgence of

¹ He declined to furnish manuscript for publication but his views were fully expressed in his book "The Primary Factors of Organic Evolution" which had just been published.

² *Proc. Am. Philos. Soc.*, Vol. 35, 1896.

³ *Proc. Am. Philos. Soc.*, Vol. 35.

those who are thoroughly familiar with the subject while I recount some of the main points in the more recent developments in our knowledge of evolution.

II

With the beginning of the present century the study of evolution entered upon a new era. Up to the year 1900 it had been based largely upon observations and what were supposed to be logical deductions. Really students of evolution were dealing with probabilities of a higher or lower order and no certainty could be reached on such a basis. What seemed highly probable to one person seemed very improbable to another. Cope accepted all the Lamarckian factors, Romanes rejected use and disuse but accepted the others, Weismann rejected all of them. The fact of evolution was accepted by practically all scientists, but the factors of evolution were largely matters of opinion, and in general persons believed what they preferred to believe. Indeed this whole subject had become so speculative that it seemed to be a field for the exercise of the imagination rather than of scientific research, and one of the eminent younger biologists, disgusted with this flood of speculation, announced, "I am done with this entire phylogeny business."

Then in 1900 Mendel's principles of heredity, which had remained unrecognized for thirty-five years, were rediscovered and a new science of accurate, experimental knowledge of heredity was born and was christened "Genetics" by Bateson. Almost at once many perplexing problems of heredity were solved; "prepotency" was found to be Mendelian dominance, "reversions" or "atavism" were the reappearance of Mendelian recessives, the results of hybridization were no longer unpredictable and the laws of heredity were at last in process of being discovered.

One year later (1901) De Vries published his great work on the mutations of the evening primrose, *Oenothera lamarckiana*, upon which he had been engaged for fifteen years and in the course of which he observed under rigid experimental

conditions among the offspring of this one species the appearance of nine constant mutants, three inconstant and one infertile mutant which differed so much from the parent form and from one another that he called them elementary species, and maintained that they furnished actual, living evidence of experimental evolution. Galton had previously (1892) expressed his belief that "sports," or sudden variations, were the real steps in the evolution of species and Bateson had published his great work on "Discontinuity in the Origin of Species" in 1894, but long before this. Darwin had given it as his opinion that evolution had occurred by means of minute variations rather than by "sports," and in this he was followed by Cope and practically all other paleontologists. Consequently it was not until De Vries had actually demonstrated the sudden appearance of mutations in his cultures that this method of evolution was widely accepted. Since then mutations have been found in almost all organisms that have been carefully studied through successive generations, and in spite of occasional objections on the part of paleontologists or other naturalists who are unable to carry on breeding experiments with their materials, the mutation theory of evolution is now well established, although it is known that mutations may be small as well as great. However, mutations are always inherited, that is they represent changes in the germ plasm, whereas changes which first occur in developed organisms are not inherited and are called fluctuations. This is indeed the chief distinction between the old evolution of Lamarck and Darwin and the new of Weismann and De Vries; in the old, attention was fixed upon the developed organism and evolutionary changes were supposed to be first made in the adult and then by some mysterious process to be transferred to the germ cells; in the newer views of evolution changes are first wrought in the germ cells and only later appear in the developed organism.

In 1903 Johannsen found that by continued breeding and isolation of self-fertilized beans he could isolate from a so-called pure garden variety, nineteen different "pure lines"

and that further selection within any one of these lines was without effect. Other similar results in a large variety of plants and animals led to the conclusion that neither artificial nor natural selection could have the effect, which Darwin had postulated, in building up a species from small variations. By some this was hailed as the "death of Darwinism," or natural selection, as a factor in evolution, but it was soon seen to apply only to fluctuations and not to mutations. It is true that selection cannot create mutations but it can act upon mutations that are offered and recent work in the field of genetics has shown that it is a potent factor in evolution.

Almost coincidentally with the rediscovery of Mendelism and the establishment of the mutation theory came the discovery of the cellular basis of these phenomena in the germ cells. The work of certain European biologists had previously furnished evidence that the inheritance material is located in the nuclei of the germ cells and chiefly if not entirely in certain threads, called chromosomes, that are found in those nuclei. When egg and sperm unite in fertilization their chromosomes commingle but retain their individual identity and in the repeated divisions of the fertilized egg, which lead to the developed animal or plant, every chromosome in every nucleus splits lengthwise and its halves separate, going into the two daughter cells; this is repeated at every cell division until every cell of the developed organism has half of its chromosomes from the egg and half from the sperm. Finally when this adult organism in turn forms eggs or spermatozoa the number of chromosomes in these sex cells is reduced to half those present in all other cells. And when the chromosomes of egg and sperm unite in fertilization the full number is again restored. Since on the average organisms inherit as many traits from one parent as from the other and since they receive an equal number of chromosomes from each parent it seemed highly probable that the chromosomes contained the inheritance material, but at the beginning of this century no one had demonstrated any genetic relationship between any particular chromosome in a germ cell and any particular developed character.

Then about the beginning of this century Professor McClung, now at the University of Pennsylvania, found that an "odd" or "accessory" chromosome is present in the males of certain grasshoppers, and in one of the last cell divisions leading to the formation of spermatozoa this chromosome did not divide but went into one cell but not into the other and thus two kinds of spermatozoa were formed, one containing the accessory chromosome and the other lacking it. Since these two kinds were equal in numbers, and since on the average males and females are equal in numbers, McClung in 1902 suggested that this accessory chromosome was the determinant of sex.

In keeping with the predominance of men, and of male psychology, in science it was but natural that it should have been assumed that this accessory chromosome would not be found in females and that its presence in males represented the initial cause of male superiority. But alas for this pleasing fiction! Professor Wilson of Columbia University and Miss Stevens of Bryn Mawr College independently demonstrated in 1905 that there are two such chromosomes in the females of certain insects and only one, or one and a fragment of another, in males. This difference in the chromosomes of males and females was later found in many other species including man. In short the male generally lacks certain hereditary materials which the female possesses and instead of woman being the lesser man, as Tennyson expressed it in "Locksey Hall," man was found to be in this respect the lesser woman. Thus the initial cause of sex, which had been a subject of speculation for thousands of years, was found in a difference in certain chromosomes in the two sexes.

A study of the method by which the usual number of chromosomes is reduced to half in the egg and sperm led to the discovery of the causes of Mendelian heredity. In 1901 the late Professor T. H. Montgomery of the University of Pennsylvania found that chromosomes of maternal and paternal origin unite in pairs just before the last cell divisions leading to the formation of the sex cells, and in 1902 Sutton,

a student of McClung's and Wilson's, discovered that corresponding chromosomes from the father and mother come together in pairs, just as corresponding fingers of the right and left hands meet when the hands are pressed together, thumb to thumb, index to index, etc. In the subsequent cell division the chromosomes of each pair separate so that each germ cell thus formed contains only one of the chromosomes of each pair, or one half the total number. Each of the two cells formed by this reduction division contains one set of chromosomes, like the set of fingers on one hand, but unlike the fingers which are permanently attached to the hands, the chromosomes are free to change hands so that one germ cell may contain a thumb chromosome from the father an index from the mother, etc., while the other cell contains corresponding chromosomes from the other parent. This union of parental chromosomes into synaptic pairs and their subsequent separation in the reduction division exactly parallels the phenomena of Mendelian segregation of characters, and there is no doubt that it is the cause of Mendelian inheritance.

With these discoveries the foundations were laid for the marvelous developments of cytology in relation to genetics which have characterized the last thirty years. Thus within the first five years of this century were established the Mendelian law of heredity, the mutation theory of evolution, the inability of selection to build up species from fluctuations, and the chromosomal mechanisms of sex determination and heredity.

III

Upon these foundations the study of evolution has advanced with giant strides during the past twenty-five years. This is especially true of the correlation between mutations, or inherited variations, and the constitution of the germ cells. Indeed this correlation has given us for the first time an understanding of the mechanisms of heredity, mutation and evolution.

Imagine the amazement and incredulity of the naturalists of a former generation, who thought of evolution only as the

transformations of developed organisms under the influence of changing environments, if they could learn that today the problems of evolution center largely in the structures and functions of germ cells! And yet this is strictly and literally true. The germ cells are the only living bonds not only between generations but also between species, and they contain the physical basis not only of heredity but also of evolution.

In the microscopic chromosomes which are found in the nuclei of all cells, and in the ultra-microscopic inheritance units or genes which lie in those chromosomes are found the earliest causes of heredity, sex, mutation and evolution. In biology as also in physics and chemistry the ultimate causes of phenomena are found not in gross bodies but in their minutest constituents. What molecules and atoms and electrons are to the physicist and chemist, chromosomes and genes are to the biologist. Present problems of evolution are not how one fully developed organism is transmuted into another, for this never happens, but rather how one type of chromosome or gene is transformed into another—not so much the effect of natural selection in eliminating certain adult forms and preserving others, although this does occur, as its much greater effect in eliminating certain types of embryos, germ cells and genotypes.

No longer do biologists discuss how adult characters can be crowded back into the egg, nor how characters acquired by an adult can be inherited, for they are almost unanimously agreed that these things never happen, but rather how changes in chromosomes and genes are produced and how they give rise to changes in the developed organism. This revolution in the study of evolution had its remote beginnings in the nineteenth century but its most significant results are confined entirely to the present century, most of them to the past twenty years.

It is impossible in the brief time at my disposal to deal with all of the significant advances of these recent years in the study of evolution, and I must of necessity select only a few for presentation. Perhaps the most significant of these

discoveries relate to the causes of mutations. In general it may be said that they are caused (1) by changes in the numbers and associations of whole chromosomes, (2) changes in the composition of individual chromosomes, and (3) changes in the genes themselves. De Vries did not attempt to trace the mutations of his evening primroses to the chromosomes, but other younger persons, many of them Americans, did this, and they found that the original form, *Oenothera lamarckiana*, has 14 chromosomes, whereas there are 15 chromosomes in seven different mutants—among them *O. lata*, *O. albida* and *O. scintillans*, while in *O. gigas* there are 28 and in *O. semigigas* 21. Since there are typically 7 chromosomes in each of the male and female sex cells of *O. lamarckiana* it seemed probable that these mutants were produced from sex cells some of which had more than 7 chromosomes. It sometimes happens that a synaptic pair of chromosomes fails to separate in the reduction division in which case 8 chromosomes go into one sex cell and 6 into the other. If then a sex cell having 8 chromosomes unites with one having the normal number 7, a form with 15 chromosomes results and if this additional chromosome is from a different synaptic pair in different cases it would account for the differences in those mutants each of which has 15 chromosomes. Likewise if all the synaptic pairs fail to separate it leads to the production of a sex cell having 14 chromosomes and if such a cell unites with a normal sex cell with 7 it produces the mutant *semigigas* with 21 chromosomes. If both male and female sex cells fail to undergo reduction each would contain 14 chromosomes and if two such should unite it would produce the mutant *gigas* with 28 chromosomes. There are other peculiar modifications of the chromosomes of *Oenothera* that cannot be dealt with here.

Many such cases of supernumerary chromosomes have now been discovered in various plants. The reduced number of chromosomes is known as haploid ($1n$), the usual condition resulting from the union of two haploid sex cells is known as diploid ($2n$), that in which there is one additional chromo-

some is $2n + 1$, etc., that in which a diploid unites with a haploid is known as triploid ($3n$), that in which two diploid cells unite is a tetraploid ($4n$), and cells with still larger numbers of chromosomes are called in general polyploids. One of the most notable of these cases of supernumerary chromosomes has been found by Blakeslee and his associates in the numerous mutants of the common jimson (or Jamestown) weed, *Datura stramonium*. Here the typical diploid number is 24, but the addition of one or another chromosome ($2n + 1$) has given rise to twelve different mutants, while many other types are produced by the further addition or subtraction of chromosomes, as well as by the breaking in two of certain chromosomes and their recombinations, a phenomenon known as segmental interchange, translocation or "crossing over."

Haploid, diploid, triploid and tetraploid plants of one species often differ markedly in appearance and they breed true if the chromosomes from the two parents are balanced so that they can unite in synaptic pairs before the formation of the sex cells. Many true Linnaean species are known that have their chromosomes in multiples of some basic number and they have probably arisen by the multiplication of their chromosomes. For example, many species of roses, and indeed many genera of the large family Rosaceae, have chromosomes in multiples of 7, and in those genera where the basal number is 8 as in plums and cherries, or 17 as in apples, hawthorns and quinces, Darlington and Moffett have shown that this unusual number has arisen from ancestral species with 7, through non-disjunction of chromosomes at the time of cell division. In wheat, oats and barley the basal number of chromosomes is 7, while different species have multiples of this number. Different species of Chrysanthemum have chromosomes in multiples of 9; more than forty species of groundsel (*Senecio*) have chromosomes in multiples of 10; seven species of docks and sorrels (*Rumex*) also have chromosomes in multiples of 10. Many other similar cases of wild species with chromosomes in multiples of some basic number could be cited. In other native species as in the genera *Viola*

and *Crepis*, chromosomes may be in multiples of some basic number, or they may be that basic number plus one or two, as in some mutants of *Oenothera* and *Datura*.

Nearly a score of new species of plants, having all the characteristics of true Linnean species, have been artificially produced by hybridization or operations under experimental conditions with consequent changes in chromosome numbers and associations. These new species are fertile *inter se*, but are sometimes sterile when crossed with either one or both of the parent species, thus fulfilling the strictest definition of true species as laid down by many systematists. Thus Goodspeed and Clausen (1925) crossed two species of tobacco plants, namely *Nicotiana glutinosa* with 12 haploid chromosomes and *N. tabacum* with 24. The first hybrid generation normally had 36 somatic chromosomes and they were generally sterile, but one partially fertile hybrid produced second generation plants one of which was remarkably large and robust and was found to have 72 somatic chromosomes, that is it was a tetraploid or *gigas* form. This plant bred true but was sterile when back crossed to one of the parent species (Clausen 1928).

Another case of the production of a true synthetic species by hybridization and subsequent doubling of the number of chromosomes was described by Newton and Pellew (1929); two distinct species of primrose, *P. verticellata* and *P. floribunda* crossed and produced a sterile hybrid; this was propagated vegetatively for several years when it suddenly produced a fertile shoot by bud transformation which bore normal seeds and from these arose a new and fertile species, *P. kewensis*, with a tetraploid number of chromosomes.

Lindstrom (1932) cut off the tops of young tomato plants of the species *Lycopersicum pimpinellifolium* and in the callus that formed, chromosome doubling took place in some of the cells, and from these cells some tetraploid sprouts arose and bore fruit and seeds. These were highly fertile and have produced plants so different from the original stock that they should be classed as a new species, especially as they are cross-sterile with the parent species.

Another new species produced by hybridization is the pink chestnut, *Aesculus carnea*, from a cross between *A. hippocastanum* and *A. parva*, the former with 20 small chromosomes, the latter with 20 large ones, while the new species has 20 large and 20 small chromosomes, or 40 in all (Hurst, 1932).

Still more remarkable are the results of crossing distinct genera of plants such as the common radish, *Raphanus sativus* and the cabbage, *Brassica oleracea*, each with 9 haploid chromosomes leading to the production of a new tetraploid genus *Raphanobrassica* with 36 chromosomes (Karpechenko, 1929); or the formation of a new genus *Triticale* by crossing wheat, *Triticum vulgare*, and rye, *Secale cereale* (Levitsky and Benetzkaja, 1929).

All of the preceding cases have to do with the production of new mutants or true species by changes in the numbers and associations of whole chromosomes. A second class of mutants are caused by changes in the composition of individual chromosomes. The members of synaptic pairs of chromosomes sometimes twist round each other, break and reunite so that portions of chromosomes become interchanged; this is known as "crossing over"; or portions of a chromosome may become detached and united to another chromosome, which is known as "translocation"; such changes in the composition of chromosomes lead to many complicated mutations which cannot be described here.

All changes in the numbers or constitution of chromosomes are known as chromosome mutations or better, permutations. Another and perhaps the most important class of mutations are those caused by changes in the ultramicroscopic genes which lie in the chromosomes. Such mutations have been found in almost all animals and plants that have been bred in large numbers under experimental conditions. The most used animal for these experiments is the little vinegar fly *Drosophila melanogaster*. Indeed in the field of heredity and evolution this is the most famous animal in the world, and the man who has been the leader in its study, Professor T. H. Morgan, has recently received the Nobel Award in

recognition of the importance of his work. Scores, if not hundreds, of different workers have been engaged in the intensive study of this little gnat and they are sometimes facetiously called Drosophilists or modern worshippers of Beelzebub, the god of flies. The peculiar advantages of this animal for the study of heredity and mutation are: (1) the ease with which it can be kept and bred in great numbers in milk bottles, (2) the fact that a new generation can be obtained every twelve days, (3) the large number of hereditary characters that can be recognized superficially, (4) its relatively small number of chromosomes, 4 pairs, that can be readily distinguished one from another, (5) finally more than 500 mutations have been found in some 25 millions of these animals that have been studied during the past 25 years. These mutations affect every part of the fly, such as color and form of body, wings, eyes, bristles, length of life, viability, liability to disease, etc. By several ingenious methods, which time does not permit me to describe, it has been possible to locate the particular genes that have undergone mutation in particular chromosomes and even in particular regions of those chromosomes, so that chromosome maps have been constructed giving the locations of these mutant genes in the different chromosomes.

These mutations seem to go in all *possible* directions, but not in *all* directions. Most of these mutant flies are less viable than the wild stock from which they came and many are lethal, that is they kill their possessor sooner or later, but a few of them are progressive. They may occur in germ cells or in somatic cells. In short wherever there are genes these may undergo mutation, but in order to be inherited they must be found in germ cells. The fact that most of these mutations are degressive rather than progressive has led some persons to doubt whether they can be the materials for evolution, but it is necessary to remember that much evolution has been degressive and the small number of progressive mutants as compared with the multitude of regressive ones teaches us at what a price progress has been bought.

IV

The nature of the changes in genes by which mutations are caused is unknown, but it seems probable that it is some kind of physical or chemical change. The fact that it may affect one gene and not another similar one that is not more than one thousandth of a millimeter away would seem to indicate that it is not some general environmental influence. This consideration led Muller (1927) to the conclusion that it might be due to some form of radiation similar to those by which physicists knock electrons out of atoms. Consequently he subjected *Drosophila* to x-rays and found that the frequency of mutation was increased about 150 times. Some of these mutants were of the same type as were previously known, but many were new. Most of them were detrimental, and more than half of them were lethals, but some of them were carried through 50 generations without reverting. In addition to gene mutations, x-rays cause breaks and translocations in chromosomes, which in turn cause marked changes in the developed animals.

A similar increase in mutation has been caused by x-rays in the case of barley, corn, the jimson weed, a wasp, *et al.* They have also been induced by radium and possibly by cosmic rays. But mutations are far too common and x-rays and radium far too uncommon to warrant the conclusion that mutations are generally caused by these means.

Searching for some more common cause of mutation Goldschmidt (1929) found that by heating the eggs of *Drosophila* to such a degree as to kill most of them he obtained from the survivors two new types, and Jollos reports (1930, 1931) that larvae of *Drosophila* that were subjected to a temperature of 36° C. for 15 to 23 hours produced during eight months more than 100 mutants while not a single one appeared in his controls. Generally these mutations appeared at least five generations after the experiments, and most important of all some of them were "orthogenetic" or progressive in a definite direction. Thus for the first time, he announced, a progressive series of mutations had been called forth by a common

environmental factor. If this work of Jollos is confirmed it may well be the most important discovery made as yet regarding the method and cause of evolutionary mutation. Plough and Ives (1934), who have just this month announced the results of their repetition of the experiments of Goldschmidt and Jollos, find that six times as many mutations occur in the heated lines as in the controls, but while this proves that increased temperature is a fruitful source of mutations there is so far no confirmation that these mutations are directed. Indeed Plough and Ives expressly deny that there is any indication of orthogenetic mutations in their experiments.

Hitherto the great objection to the mutation theory of evolution has been that mutations are so generally regressive and that they lead nowhere. The only method of meeting this objection has been to rely upon natural selection to eliminate vast numbers of useless mutations and to preserve the few useful ones and thus slowly to build up the marvelous combinations of useful adaptations that all organisms possess. But there are many indications in the living world that evolution has proceeded in certain directed lines, sometimes even further than was useful, as for example in the enormous size of body and weight of armor of certain Dinosaurs and Titanotheres, and many zoölogists since Eimer have insisted that "orthogenesis" or directed evolution is a necessity. If directed mutations can be caused by some common environmental factor, as Jollos suggests, it would solve one of the major difficulties of the mutation theory. Osborn in particular has emphasized the necessity of definitely directed variations in a series of publications during the past forty years, the last of which has just been published (1934). He originally called this principle "definite variations" and later "rectigradations." More recently he has stressed the necessity not only of directed mutations but, much more, of useful and progressive mutations in any explanation of evolution. This principle of the *origin* of the fittest, as contrasted with the *survival* of the fittest, he calls "aristogenesis" (1933, 1934).

Goldschmidt (1933) has recently emphasized the importance of certain embryological processes in evolution. He concludes that genes control development partly by influencing the velocities of certain reactions, and he suggests that by changing the differential growth rate at an early stage a perfectly new evolutionary line could be started. This suggests a speculation which I advanced before this Society in 1903, and published in greater detail in 1905, regarding the origin of major groups, or phyla, of the animal kingdom. The older evolutionists, for example, undertook to show by what transformations of the developed body an annelid or arthropod could be converted into a vertebrate. It was supposed that the invertebrate turned upside down, its mouth closed up and a new mouth formed, and many other changes occurred which would be absolutely impossible in any developed animal. Similar impossible translocations of organs of adults had been proposed to explain the origin of inverse asymmetry, as for example in those rare cases in man where the heart is found on the right side instead of the left and all other asymmetrical organs are reversed in position. When it was discovered that such inversions of all the organs of sinistral as compared with dextral snails could be traced back through the embryology to the early egg cell, it was evident that this inversion was due to relatively slight changes in the locations of substances in a single cell; such changes are now known to be caused, in the last analysis, by genes. Similarly when it was discovered that the location of the principal organs of several different phyla could be traced back to the pattern of localization of special substances in their eggs, I suggested that relatively slight changes in the localization of these substances would bring about the characteristic differences in the location of the organ systems of vertebrates as compared with invertebrates. Thus instead of turning a developed worm or arthropod upside down, and making many impossible translocations of its organs it would be relatively simple to convert one type into another by translocations within a single cell, such changes ultimately being caused by

gene activity. Unfortunately this suggestion, like that of Goldschmidt just mentioned, is at present without experimental proof.

V

Adaptations have always been the chief marvel of the living world and their method of origin is still the greatest problem of biology. The only natural explanation that has as yet been established is Darwin's principle of the elimination of the unfit and the survival of the fit. There is abundant evidence, both observational and experimental, that this principle is true, but when we load upon it the obligation of explaining all the marvelous adaptations and combinations of adaptations that every living thing possesses the doubt arises as to whether this principle alone can support the enormous burden. I have long felt, along with Cope, Osborn and many others, that some additional factor is needed to explain such universal adaptations. And Darwin himself felt the force of this for he said that he never thought of attempting to explain the origin of such a complex and wonderfully coordinated structure as the eye without a shudder. He sought refuge, as did Cope and many others, in the inherited effects of use and disuse as an aid to natural selection, but this refuge is now denied us, for the evidences from genetics are conclusive that such effects are not inherited.

A solution that has found favor with many geneticists lies in the vastly greater duration of past time than was formerly allowed for organic evolution. Darwin estimated that past evolution must have required something like 400 million years. Lord Kelvin speaking for the physicists of his day would allow him not more than 100 million years. But the physicists, astronomers and geologists now say that the earth was ready for life at least 1000 million years ago, and geneticists console themselves with the thought that given almost infinite time and almost an infinitude of mutations almost anything could happen. But after all they cannot help feeling that this is not a satisfactory solution of the vast problem of fitness—at present by far the greatest problem of biology.

Another possible solution of this problem was first pointed out by Weismann in his doctrine of intrapersonal selection and a modification and modernization of this is found in R. A. Fisher's and J. B. S. Haldane's application of selection to genes and genotypes.

I have proposed (1921) a still further application of the selection principle to all the reactions of living things. We know that all organisms are differentially sensitive, that is they move or grow toward certain sources of stimuli and away from others, and in general they respond positively to stimuli which we would call pleasant or satisfactory and negatively to those which we call unpleasant or unsatisfactory. In short they are generally able to differentiate and select between that which is satisfactory and that which is not. No one can at present explain this property of life, but apparently it is a general characteristic of all living things. It characterizes the behavior of germ cells and embryos as well as adult organisms. It is the basis of that form of behavior known as "trial and error"; it is fundamental to all learning and is the beginning of intelligence and wisdom in man as well as in higher animals. This capacity to differentiate and select is not unlike the "archaesthetism" of Cope and it is at bottom an extension of the selection principle to the reactions of organisms—but with this difference, that whereas in Darwinian selection the selector or eliminator was found exclusively in the environment, in this conception the organism itself also selects or eliminates. There is no mechanistic explanation of this property of life but the same is true of many other properties of living things. Because we cannot at present explain mechanistically the properties of the organization of protoplasm and its capacities of assimilation, reproduction and sensitivity is no ground for denying that these properties exist, and the same is true of the property of organic adaptation. But given these properties, science can explain in a mechanistic, that is, in a causal manner, multitudes of structures and functions and reactions that have arisen in the course of evolution.

It seems to me that recent theories of evolution have too often left out of account these fundamental properties of life. Assigning all evolution to externally caused mutations and to environmental selection neglects the fact that the organism is itself a living, acting and reacting system. Life is not merely passive clay in the hands of environment, but is active in response to stimuli; it is not merely selected by the environment but is also itself ever selecting in its restless seeking for satisfaction. Macfarlane (1918) has called this property of organisms "proenvironment" and has assigned to it an important function in evolution. Cuenot (1911) has shown that many animals seek and find by a process of trial and error those environments for which they are by nature best adapted and he calls this "preadaptation." By a similar process, namely the elimination of unsatisfactory responses, most of the individually-acquired adaptations of organisms may be explained. Such acquired adaptations as the repair of injuries, the regeneration of lost parts, acclimatization to high altitudes or temperatures, neutralization of poisons and immunity to disease, which were at one time hailed as a "death-blow to Darwinism," may be explained by an extension of the Darwinian principle of the elimination of the unfit to the multitudinous reactions of organisms.

From my earliest introduction to the science of biology I have been an admirer of August Weismann. Of late it has become fashionable to decry the speculations and theories of Weismann since they were not based on experiment. But no one can truthfully deny that his logical deductions were a powerful stimulus to research and that many of them have been confirmed in a truly remarkable manner by recent work. He maintained, long before it was demonstrated by genetics and cytology that the hereditary substance consists of discrete particles, his determinants, arranged in a linear series in the chromosomes. His prediction that one of the maturation divisions in the formation of the egg and sperm must lead to the reduction of the chromosomes in those cells to one-half the number present in somatic cells was almost as brilliant

an example of scientific prophecy as was the prediction of the existence and position of the planet Neptune. And finally his explanation of the origin of fitness in the living world is still, I think, the best scientific conception that has ever yet been offered. I cannot better express my own views on this subject than by closing with these words from the preface of his last book (1902): "Although I may have erred in many single questions which the future will have to determine, in the foundation of my ideas I have certainly not erred. The selection principle controls in fact all categories of life units. It does not create the primary variations but it does determine the paths of development which these follow from beginning to end, and therewith all differentiations, all advances of organization and finally the general course of development of organisms on our earth, for everything in the living world rests on adaptations."

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all like gelatine or egg albumin. That indeed is why it is alive.

If one is to learn their secrets, one must study living cells directly, and one must study them while they are alive. Within the last generation, methods have been devised for such study. It is now possible to determine with a fair degree of accuracy, at least some of the physical properties of protoplasm.

Living cells are surrounded by osmotic membranes which offer resistance to the passage of water and dissolved substances. The permeability of these membranes can be measured, and there is a large literature on this subject. Many authorities believe that the primary effect of stimulation is to increase the permeability of the cell membrane, and that anæsthetics have the opposite effect. This point of view has had a tremendous influence, although no one has ever clearly shown why stimulation should increase permeability, or how such an increase would affect the protoplasm within the cell membrane. The theory has had more support from studies of stimulation than from studies of anæsthesia. Often enough anæsthetics are found to have either no effect on the permeability or to have an effect in the wrong direction.

But whether or not the permeability of the cell membrane is altered by anæsthetics, certain it is that the interior of the cell is profoundly modified. Within recent years it has been possible to measure the viscosity of the protoplasmic fluid. About twenty years ago I was able to show that whenever sea-urchin eggs were stimulated, their protoplasm changed from a fluid to a much more nearly solid condition. And this gelating effect of stimulation could be prevented completely by ether and other fat solvent anæsthetics.

Since these early studies of twenty years ago, I have learned much that I did not then know about the effects of stimulation and anæsthesia on protoplasm. The story is a very complicated one, and I will stress only one aspect of it.

If it be true that stimulating agents cause a stiffening or gelation in protoplasm, how can we interpret such a phe-

nomenon? Ordinary colloids do not become coagulated or gelled by weak electric currents, by ultraviolet rays, or by a slight mechanical impact. The unique behavior of the protoplasmic colloid demands some sort of an explanation.

When protoplasm is excessively stimulated whether by the electric current, ultraviolet rays, mechanical shock or by chemical treatment, it typically becomes transformed into a foamy mass of vacuoles. The earliest student of protoplasm, Dujardin, had some conception of the vacuolization reaction and its importance. But since his day, with the wide growth of our biological knowledge, and with the greater emphasis on the study of killed and fixed material, the importance of vacuolization has been largely lost sight of. True, students of the effects of radiation have repeatedly described vacuole formation, but almost no one has coördinated this information. Loeb in his famous studies of artificial parthenogenesis stressed the fact that over-stimulation of sea-urchin eggs resulted in what he called cytolysis, and he figured this cytolysis reaction as involving the appearance of vacuoles within the protoplasm. A few years ago, in a monograph on the colloid chemistry of protoplasm, I gathered together numerous descriptions of vacuole formation in all sorts of protoplasm. Apparently, whenever protoplasm is stimulated and this stimulation is carried to excess, new films and vacuoles appear in the cell.

Neither Loeb nor his predecessors had any clear idea of the nature of this vacuolization reaction. Only rarely has it been studied experimentally. One method of approach is to study what happens when a cell is torn or broken. If a sea-urchin egg cell is crushed so that protoplasm begins to flow out, there is a sharp reaction at the border of the emerging droplet. This I have called the surface precipitation reaction. A new membrane or film is formed so as to enclose completely the escaping protoplasm. If the crushing has been vigorous, not only does a film form at the border of the droplet, but new films appear throughout the fluid which has emerged from the cell. This becomes filled with vacuoles, and under

certain conditions, the vacuole reaction spreads throughout the entire cell so that it is completely full of these structures, and presents the appearance which Loeb called cytolysis. Now the important point for our discussion is that the formation of the film at the border of the escaping droplet and the appearance of vacuoles within the interior are all due to essentially the same reaction. This is indicated by the fact that whatever prevents the appearance of the outer film also prevents the appearance of vacuoles.

In earlier papers I have shown that the reactions we are considering are essentially similar to the reactions which occur when blood coagulates or clots. Both the surface precipitation reaction and blood clotting depend on the presence of calcium. Both are inhibited by excess of salt. And there are various other resemblances which there is no time to discuss.

The clotting of blood is an extremely complicated reaction or series of reactions. It involves a number of substances not well understood chemically. Recent work of physiological chemists has shown that all these substances are to be found in ordinary cells like muscle cells. Granted the presence of all these clotting substances in a cell, is it not logical to assume that under proper conditions they would interact?

Let us assume therefore that the gelation or coagulation involved in protoplasmic stimulation is fundamentally similar to blood clotting. If we make this assumption, we are immediately in position to explain some remarkable developments in modern physiology. Within the last few years, a whole new branch of study has been developed which is known as humoral physiology. In this field the essential fact is that cells when aroused to activity produce substances which are capable of arousing other cells. Thus if one stimulates heart muscle cells, they give off substances which affect other heart muscle cells. This is but one example of many. The point I wish to make is that such a phenomenon finds an analogy in the ordinary facts of blood clotting. If

one drop of blood clots, it produces a substance, thrombin, which is able to cause clotting of a second drop of blood which ordinarily would not clot. It is interesting to note further that the substances of clotted blood may act directly as stimulants of muscle (O'Connor, 1911), and that moreover, anti-coagulants prevent such stimulation (Kraus and Fuchs, 1929).

I come now to the main point of my paper. If it be true that stimulation of protoplasm causes something akin to a blood clotting reaction within it, then if the theory is complete it should be possible to show why and how anæsthetics prevent the reaction. This it is rather easy to do in the case of the salt anæsthetics, magnesium and potassium. They may perhaps replace the calcium of the cell, and inasmuch as the protoplasmic clotting reaction requires calcium, in the presence of magnesium and potassium, it is easy to see why the cell can be anæsthetized. The other common cation, sodium, ordinarily does not act as an anæsthetic, for queerly enough when cells are bathed in pure sodium solutions, the bound calcium within them is set free. This has been shown by the recent work of one of my students (Mazia, 1933).

The effect of ether and fat solvent anæsthetics is much more difficult to interpret. Although, indeed, it is easy enough to show that solutions of ether actually cause liquefaction of the protoplasm and prevent the gelation which accompanies stimulation, it is not so easy to relate the liquefying action of the ether to the type of clotting reaction which we have been considering. If the clotting reaction is essentially the same as the reaction which occurs when a cell is torn, that is to say if it is essentially the same as the surface precipitation reaction, then it should be possible to show that suitable concentrations of ether do have an inhibiting effect on the surface precipitation reaction.

However, when one crushes a sea-urchin egg in a solution of ether in sea-water, the surface precipitation reaction is apparently not at all affected by the anæsthetic. Year after year I have tried to discover some effect of ether on this

reaction, but always without success. If the point of view I have been urging be correct, this seemed like an insurmountable difficulty.

Then last summer it occurred to me that when ether acted as an anæsthetic on the intact cell, it was acting on a system which differed in one essential respect from the torn or broken cell. Within the protoplasm, there is only a trace of free calcium ion, practically none at all, whereas when a cell is torn in sea-water, calcium is present in abundance. It may be remembered that calcium is the essential ion in the surface precipitation reaction. It might then be possible that ether would prevent the reaction under conditions like those in the cell interior, where there is an extremely low concentration of calcium.

Actual tests proved this to be true. If sea-urchin eggs are crushed in solutions containing one part of isotonic calcium chloride solution to two or three thousand parts of isotonic sodium chloride solution, a clear surface precipitation reaction occurs. But now if two or three per cent of ether is added to the solution containing this small percentage of calcium, the eggs when crushed show no surface precipitation reaction whatever. Thus in the presence of only small amounts of calcium, the surface precipitation reaction is inhibited by ether.

The effect of ether on the surface precipitation reaction is shown even more clearly by the protoplasm of the single celled animal *Stentor*. This form gives a very beautiful reaction when it is crushed. As the protoplasm flows out of the wound, its border is clotted or precipitated to form a new film or membrane. Vacuoles can also be observed within this membrane. Now if the *Stentor* is crushed in the presence of one per cent ether, neither film nor vacuoles appear, and the protoplasm flows freely through the surrounding medium. Similar effects are produced by other fat solvent anæsthetics.

It has thus been possible to show that ether and other fat solvent anæsthetics do actually inhibit the surface precipitation reaction and the clotting reaction which depends on it.

This provides an additional step in the attempt to interpret stimulation and anæsthesia in terms of the colloid chemistry of protoplasm.

When protoplasm stiffens or clots, the reaction involved is an extremely complex one. These are factors which favor clotting, others which tend to retard or reverse it. In my discussion, I have been forced to omit many complicating details, and there are numerous others still to be worked out. For the sake of simplicity, I have tried to present a general point of view rather than a mass of specific facts. The theory that I have outlined is far from complete. But even as it stands, it may go farther towards an explanation of the manifold phenomena of stimulation and anæsthesia than any other type of interpretation.

THE LONG VOYAGES OF THE POLYNESIANS

ROLAND B. DIXON

(Read by title, April 21, 1934)

THE Polynesians have long had the deserved reputation of having been the most skillful and daring of navigators among primitive peoples. Their prowess in making voyages of amazing length has been justly acclaimed, but some enthusiastic writers by what I believe to be a misinterpretation of the traditional records and a failure to give them any sort of critical consideration, have often exaggerated the facts. It is the purpose of the present paper to set these in their true light and to discuss briefly some of the implications to which they give rise.

Two types of vessels were in use by the Polynesians for long distance voyaging (1) the single outrigger canoe and (2) the double canoe. Traditionally it was the latter type which was by far the more commonly used. The reason for this was that although more difficult to handle in bad weather, it was on the whole more seaworthy, and in spite of its slower speed, provided a very considerably greater carrying capacity. A few traditional voyages of considerable length were, however, definitely stated to have been made in outrigger canoes.

In size the double canoe varied not a little in different parts of Polynesia. In Samoa, Tonga, the Society group and Hawaii the average over-all length of the canoe bodies seems to have been between thirty and sixty feet, the extreme limits running up to eighty or even ninety feet. In the Marquesas and western Tuamotus the maximum seems not to have exceeded fifty feet, whereas in the eastern Tuamotus and Easter Island the largest craft were less than half this length. The two bodies of the double canoe were firmly joined together by a series of heavy cross beams, the bodies being spaced

three or four feet apart in the smaller and six or seven feet in the larger vessels. On these cross-beams a platform was laid, which thus in the case of the largest double-canoes would be some eighty feet long and about sixteen to eighteen feet wide, if we assume the maximum breadth of five feet for each canoe body. For war purposes these largest vessels are said to have accommodated as many as a hundred or more warriors, but for long distance voyaging the space needed for supplies reduced the effective carrying capacity considerably. Some of the traditional statements therefor to the effect that several hundred persons were carried on long voyages in a single canoe, are obviously not possible of credence.

A word might be said in regard to rafts. In Mangareva and the Gambier group, little if any use was made of canoes, at least at the time of the first European contacts. In their place, small sailing rafts were employed, accommodating perhaps a score or so of persons. Some myths and traditions have, however, been recently secured here, which speak of rafts several hundred feet square and carrying a thousand people. It need hardly be pointed out that such craft could not possibly have been practical, and these incredibly large rafts and the equally incredible voyages said to have been made on them, are clearly imaginary. The tales have, however, been naively accepted as genuine by their all too credulous collector.

If we turn now to the question of the actual length of voyages made, it appears that they fall naturally into two classes, those made since European contacts and so definitely known, and those recorded only in tradition and myth. In the eighteenth century and apparently for several centuries before, really long voyages seem to have gone out of fashion, for Hawaii in the north and New Zealand in the south had been isolated and without any contact with other groups for some hundreds of years. In the rest of Polynesia occasional trading and other voyages between neighboring island groups appear to have been all that was attempted, and none of these involved an open sea distance of much more than five

or six hundred miles. Actually attested voyages thus during the last two centuries have not exceeded a moderate length.

The oldest voyages recorded in myth and tradition are those vaguely mentioned in strictly mythological tales and referable perhaps to the earliest period of exploration and settlement of Polynesia. They are, however, so overlaid and modified by supernatural elements that they have little or no value for our purpose. The more definitely traditional voyages on the other hand are in a very different category. They deal with the period of renewed maritime activity lasting from about the tenth to the fourteenth centuries, during which daring expeditions for trade, conquest, colonization and other purposes extended from one end of Polynesia to the other. The majority were made from the Society group as a center. Traditional heroes sailed hence westward to Samoa, Tonga and Fiji, northward to Hawaii and eastward to Easter Island, while explorers and colonists sailed southwest to New Zealand, bringing thither the basis of its historical Maori population. Unfortunately the records of these long voyages are in most cases brief in the extreme, and it is this brevity of statement that has, I believe, led to the not infrequent misunderstanding and exaggeration of the facts. An example or two will make the problem clear.

In the traditional accounts of the adventures of Tangiia, this doughty hero after experiences in Raiatea of the Society group, is said to have "returned to Fiji." Now this statement is interpreted by some writers as meaning that Tangiia sailed direct from Raiatea to Fiji, a distance of sixteen or seventeen hundred miles. On a previous visit to Fiji however, he had gone by way of Samoa, and although on *this* occasion no reference is made to a stop over there, it is at least probable that this usual route followed by others on other occasions, was taken. In other words, the normal voyage was from group to group, thus affording opportunity for replenishing supplies. The same argument applies, I believe, to all the reputed voyages of great length, and in particular to the longest of all—that said to have been made by this same

Tangiia from Fiji to Easter Island, a distance of some four thousand miles or more. Being in Fiji, the tradition merely states that "he went right away to Rapa-nui," the native name for Easter Island. This has been accepted by some as meaning that he went direct, and they point to the words "right away" as implying this. But the context seems quite clearly to mean that the actual meaning is "at once" *i.e.* without staying longer in Fiji. Any possible direct course from Fiji to Easter Island would lead through the Tongan, Cook or Gambier groups, so that even if a "direct" voyage were attempted, familiar ports of call were inevitably on the way. Further, it is extremely improbable if not impossible that, skillful as were the Polynesians in all that pertains to navigation, a single small island could have been reached at the end of a four thousand mile voyage, with neither chart nor compass to guide. It is vastly more probable in view of known Polynesian sailing procedure, that the statement "went right away to Rapa-nui" means that Tangiia went thither forthwith by what would have been the normal route, *i.e.* from Fiji to Samoa, thence to the Cook group, the Society group and on via the Tuamotus, Mangareva and Pitcairn. Corroboration of this view is given by the fact that in going from Tahiti to Pitcairn only, the direct route was not followed, for we have accounts of two such voyages by other navigators, in which they went either by way of Hereheretue and Moruroa or by Anaa and Hao in the Tuamotus to Mangareva, and thence to their journey's end. That long voyages were habitually broken at intermediate points is further shown by the fact that all the voyages from Tahiti to New Zealand were by way of the Cook group, some even stopping between there and New Zealand at the Kermadecs.

Finally, the argument applies to the voyages between Tahiti and Hawaii, a direct distance of some twenty-two hundred miles. Here the chances that the journey was sometimes at least, broken at the Marquesas or at Malden or Fanning Islands, are strong, and indeed there is traditional evidence that such stops were actually made at Fanning. If

made direct, the voyage between Tahiti and Hawaii would apparently be the longest open sea transit for which reasonable evidence can be given. We may therefore say that the Polynesians were capable of making and did probably at times make unbroken voyages whose maximum length was something less than 2500 miles. Yet, so far as the traditional record goes, such voyages were seldom made, the journey between Tahiti and Hawaii having been recorded not more than about a dozen times in a period of four hundred years.

All the voyages hitherto discussed were voluntary, and made for exploration, trade or colonization, or by fugitives escaping from an enemy after defeat. Involuntary or storm-drift voyages have, however, often been mentioned as important factors in the spread of the Polynesian people. These refer to large voyaging canoes blown out of their course, or to smaller fishing craft carried away unexpectedly by the same agency. Since the first European contacts a considerable number of such drift voyages have been recorded. The majority of them covered a distance of less than five hundred miles, a few somewhat more than double this, and in a single instance the distance traversed amounted to about fifteen hundred miles. The greater number of all recorded drifts were from east to west, a few followed a north and south course, and only in one instance from west to east. In this case a double canoe was blown from near Tahiti to Vanavana in the Tuamotus, a distance of some six hundred miles, a third of the total of forty-eight persons on board dying of starvation before land was reached. Since the majority of drift voyages were thus from east to west, they can have played little part in the eastward spread of the Polynesian people, and in no case were such involuntary voyages as long as that from Tahiti to Hawaii.

The proven skill and daring of the Polynesian navigators within the area of Polynesia itself have naturally suggested that they may well have made adventurous voyages beyond the eastern margin of Polynesia, and so reached the shores of the New World. Such theories have been largely used in

attempting to account for the presence in America of certain cultural traits which have analogies, real or fancied, with traits in the Oceanic area. Since Hawaii and Easter Island lie within less than 2500 miles of the Californian and Peruvian coasts respectively, the voyage thither would have been within the compass of the Polynesian sailor. The Marquesas and Tuamotus lie considerably further away, and would thus probably be outside the limit of unbroken voyaging of which the Polynesians were capable. From Easter Island, an exploring voyage eastwards would bring the adventurous sailor to the Peruvian coast, and since the majority of the cultural traits whose diffusion from Oceania is assumed, are found in South America, this island has been a favorite hypothetical starting point for the theorists. That the Easter Islanders themselves should have attempted such a voyage, however, is improbable, since the island is treeless, and thus affords no supply of timber for the making of large canoes. At the time of the first European contacts, the largest canoes in the possession of the people could carry only three or four persons, and were made from such bits of drift wood as happened to come to shore. The island could thus have served as a starting point only for canoes of appropriate size, coming there from some point in Central Polynesia. Hawaii, however, was in a very different position, for here abundant timber was available for building the largest canoes, the best being derived from the great drift logs from the Oregon and Washington coasts which were occasionally brought by the ocean currents to Kauai, the northernmost island of the group. These logs moreover, would have served in themselves as indications of land to the eastward, and thus Hawaii was in a most favorable position to have been a center from which easterly explorations might have been made.

Yet the Hawaiians do not seem to have had the zest for long voyages which the people of central and western Polynesia had. At the time of the first European contact at the end of the eighteenth century, the Hawaiians had been completely out of touch with the rest of Polynesia for cen-

turies, their voyages having been confined wholly within the group. Even in the great period of adventurous sailing from the tenth to the fourteenth centuries, when most of the rest of Polynesia was being threaded by voyages hither and yon, the Hawaiians rarely went beyond the Marquesas, and most of the expeditions between Hawaii and Tahiti were carried out by Tahitians themselves. In the whole very considerable mass of their recorded traditions, there is no reference whatever to eastward questing or to any land which can reasonably be interpreted as being anywhere outside Polynesia. And there are few particularly significant cultural trait analogies between Hawaii and the Californian and adjacent coasts. The same holds true for the whole mass of Polynesian traditions of voyaging. All the questing and seeking was westward in the direction of their dimly remembered earlier home, although we have one account of an explorer who sailed south, far beyond Rapa in the Australs, where he seems to have come in touch with drifting Antarctic ice. That in all these traditions of voyaging there are none from any credible source, referring to search to the eastward, or of strange lands in that direction, seems really significant, and gives ground for the opinion that during the four centuries of active navigation and exploration reaching back from the fourteenth to the tenth century, no voyages from Polynesia to the American shores were made.

If then voyages to the New World were made, we are led to relegate them in time to the considerably earlier period when the hitherto empty islands received their first human settlers. Then when the zest for exploration was strong, when restless eastward searchings had revealed group after group further and further toward the rising sun, then we may suppose that an eager questing beyond the farthest islands reached, might have brought the dauntless sailors to the American shores. This possibility seems to be changed to a certainty by the facts in the case of the sweet potato. This plant declared by the botanists to be unquestionably a native of the New World, seems on the basis of the evidence

to have long been in use as an important food-plant by the Polynesians of eastern and central Polynesia, at the time of the first European contacts. The plant could only have reached Polynesia from America by the aid of human hands, and since we have no evidence that at any time the Indians of the Pacific Coast of South America where the sweet potato was grown, had either the craft or the skill for making long sea journeys, we are forced to conclude that the transference of the plant was carried out by Polynesians. At some time a party of these intrepid sailors must have reached the Peruvian coast, and have taken this valuable plant back with them to their island homes. That the region where the sweet potato was thus secured must have been Peru, is made probable by the fact that the basic Polynesian term for the plant "kumara," is that used by the Kechua-speaking people of Peru. The word does not occur in the southern or Incan dialect, but in the supposedly older Chinchasuyo dialect, spoken along the coast for a hundred miles or more north and south of Lima. To reach this portion of the South American coast it is clear that the Polynesian visitors must have come from Easter Island, if we are to accept a range of not much over two thousand miles as the probable limit of Polynesian voyaging. It has been shown that Easter Island seemed an unlikely place from which such a journey should originate. We are nevertheless led to believe thus, that in the early period of eager exploration, it may well have been the starting point for further eastern questing, by a group who, from further west, had in their large canoes already crossed a thousand miles of open sea to reach the lonely island.

To assert the practical certainty of at least one voyage by Polynesians from the eastern margin of their area to the Peruvian coast, is but to raise a multitude of questions and problems that cannot here be discussed. Some are practical, such as the difficulty of the return voyage from America to the homeland, for it is one thing to sail eastward and strike a continent, and quite a different one to sail thence westward and find again a single tiny island. Also there is the problem

of the character of the reception such strange visitors would be likely to get at the hands of the highly cultured and warlike people who occupied the Peruvian shores. Some of the problems on the other hand are more theoretical, and involve us in keenly disputed questions of cultural diffusion on a large scale from the Oceanic region to the South American area, and the failure of the Polynesian visitors to bring back with them other items of South American culture beside the single one of the sweet potato. That the Polynesians were capable of reaching the New World shores and doubtless did so on a very few occasions, seems to be established. It remains for competent students to study minutely and critically the many problems thus raised, in order that they may be solved soundly and rationally, rather than as has so often been the case, guessed at by enthusiastic but reckless theorizers.

NEUROHUMORS AS ACTIVATING AGENTS FOR FISH MELANOPHORES

G. H. PARKER

(Read April 19, 1934.)

THE integumentary structures whereby an animal may quickly change its tint were named chromatophores in 1819 by Sangiovanni who expressed the belief that these bodies were under the control of nerves. The chromatophores in fishes, first described independently in 1863 by von Siebold and by Buchholz, are of several kinds the most conspicuous being the melanophores, cells containing granules of the dark pigment melanin. These cells, which often have a remarkably rich system of branches, present two extreme conditions so far as their pigment is concerned. Either the particles of melanin in a given cell are concentrated about its center in which case the cell appears as a small black spot (the chromatophores of the upper part of Fig. 12) or they are dispersed through the whole of the cell including its branches and thus spread over a larger area (those in the lower part of Fig. 12). The first state may be called the concentrated one of the melanophore and induces the light condition of the fish, and the second the dispersed one inducing the dark condition. As early as 1872 Pouchet showed that the melanophores of fishes were not only under the control of nerves but of nerves from the sympathetic, or better, the autonomic system, a conclusion confirmed by many subsequent workers especially von Frisch (1910). In 1875 Bert declared in favor of a double innervation of chromatophores and this view about which much uncertainty still exists, has been advocated in recent years by not a few workers (Giersberg, 1930; Smith, 1931; Mills, 1932). Such a view would imply two sets of nerve-fibers, one probably concerned with the concentration of the pigment and the other with its

dispersion. Such fibers might be appropriately termed concentrating fibers and dispersing fibers respectively. Following the conventional classification of autonomic elements the concentrating fibers would fall under the head of the sympathetic system proper and the dispersing fibers under that of the parasympathetic. Fibers of these two kinds more or less mingled are doubtless the elements whose branches and terminals have been so adequately figured by Ballowitz (1893) in his account of the innervation of fish melanophores (Fig. 1). From the work of this investigator it is clear that in these animals each melanophore rests in a rich arborization of nerve-fibers and their terminals and is not innervated by one or possibly two nerve-endings as muscle-fibers are.

These anatomical details suggest a very concrete application of the neurohumoral hypothesis (Parker, 1932) to such functional relations as may subsist between the nerve-terminals and the melanophores which they control. Thus from the standpoint of this hypothesis the spread of melanophore pigment would be assumed to be induced by a dispersing neurohumor secreted by the appropriate nerve-endings and passed from these over the minute intervening space to the melanophore which would thereby be excited to spread its pigment. In a similar way pigment concentration would result from a concentrating neurohumor derived from the system of concentrating terminals and passed to the melanophore. Whether there is evidence in the responses of melanophores in fishes for such neurohumoral relations and particularly whether there is ground for the assumption of a concentrating neurohumor in these animals, is the subject of this communication.

In many bony-fishes it can be shown with certainty that dispersing nerve-fibers are abundantly present. If one of the radiating nerves to the tail-fin of such a fish, a catfish or a killifish for instance, is cut, immediately the area normally innervated by that nerve becomes dark and remains so for many hours or even days thereafter (Fig. 5). Such an operation induces a dispersion of melanophore pigment in the

region concerned and is believed to result from an excessive stimulation of the dispersing nerve-fibers in the nerve that is cut.

This, however, is not the only interpretation that may be offered for the condition described. It might be maintained, for instance, that the melanophore nerves exert an inhibitory influence on the melanophores, thus holding them in a condition of pigment concentration and that when these melanophores are relieved from the influence by the cutting of their nerves, their pigment immediately disperses. That such is not the case can be shown by placing on the tail of a killifish at right angles to its rays and consequently to its radiating nerves a capillary tube carrying a liquid (50 per cent alcohol) at a temperature a little below freezing (Fig. 2). By thus chilling the nerve, transmission over it is temporarily abolished. Hence inhibitory impulses, if present, would be blocked as though the nerves had been cut and the associated melanophores might be expected, if the assumption of inhibitory action were correct, to show dispersion of pigment. As a matter of fact no such change is seen though the nerves at points proximal and distal to the cold-block can still be shown by pigment dispersion to be open to normal activation. It may, therefore, be definitely concluded that dispersing fibers are not inhibitory fibers, but fibers concerned with the direct excitation of the dispersion phase. Their severance excites this type of activity to an excessive degree.

The dark band formed by the dispersion of the melanophore pigment in the area innervated by the cut nerve appears within a minute or two after the cut is made and, having arrived at a maximum, it subsequently gradually fades till after one to several days it attains the same light tint as that of the fish itself (Figs. 5, 6, 7, 8). That the loss of tint here noted is not due to the degeneration of the nerve-fibers is shown by the fact that if the nearly faded band is recut in a region distal to the first cut, its activity is fully demonstrated by a quick revival of considerable depth of color. Such observations show quite conclusively that the radial

nerves of the tail-fin in these fishes, the catfish and the killifish, carry nerve-fibers whose normal action on their associated melanophores is to produce a dispersion of pigment.

Is there any evidence that these fibers act by producing from their terminals a dispersing neurohumor? Such a neurohumor to reach the melanophore must pass over the small space, probably lymph-filled, between the nerve terminals and the melanophore. Such a neurohumor might, therefore, well make its way into the blood of the fish. To test this possibility defibrinated blood from dark fishes was injected into light ones with a view of exciting a local dispersion of melanophore pigment. In one set of tests the blood was taken from dark killifishes in which the pituitary gland has been shown to have no functional significance for change of tint (Matthews, 1932). In another set (Parker, 1934) it was taken from catfishes from which the pituitary gland had been removed. In both instances the blood from the dark fishes produced no darkening in the light ones, though when the defibrinated blood from a dark frog is injected into a light one the light frog becomes quickly and noticeably dark. It might therefore be concluded that though such fishes as the catfish and the killifish possess dispersing nerve-fibers these fibers do not act by means of a corresponding neurohumor.

Such a conclusion, however, is not necessarily warranted as the following observation shows. If in a catfish an almost fully faded caudal band is flanked on each side and for about half its length by newly excited dark bands, the part of the faded band proximal to the flanking dark bands remains indefinitely light whereas that between the two bands gradually darken (Fig. 3). This is very clearly seen in the condition of the pigment in the melanophores of the several regions. In the light part of the faded band the pigment is concentrated as in the melanophores of the body of the fish in general (Fig. 9). In the dark part of the faded band the pigment is largely dispersed (Fig. 10) though not so much so as it is in the dark flanking bands (Fig. 11). It is difficult to explain

this darkening of the distal portion of the faded band except on the assumption that a dispersing neurohumor has made its way from the flanking dark bands into the faded one. Such a passage, as the agreement of the affected area with the flanking bands shows, seems inevitable and yet the blood of such fishes appears to contain no such dispersing neurohumor. Notwithstanding this fact I believe that such a transfer has been accomplished but by way of the cells and not by the blood. In my opinion the neurohumor concerned is not a water-soluble substance but one that dissolves in oil and hence is open to transfer from cell to cell over the lipid surfaces of these elements. With such an assumption it is easy to understand the conditions already described and further the slowness of the transfer, a matter of hours instead of minutes as in transportation by blood, is fully accounted for. I, therefore, conclude, notwithstanding the fact that the defibrinated blood of properly prepared dark bony-fishes does not darken light ones, that the dispersing nerve-fibers of these fishes act on their associated melanophores by means of a dispersing neurohumor.

The presence of concentrating nerve-fibers and neurohumors whereby bony-fishes attain a light coloration is much more difficult to settle than that of the dispersing elements just discussed. That concentrating fibers occur in certain fish such as the dogfish, where nerve severance excites a light band, has already been established (Parker and Porter, 1934), but such a condition in an elasmobranch does not prove the existence of a similar state in teleosts. Among bony-fishes so far as is known nerve cutting is never followed by light bands but always by dark ones. The presence of concentrating fibers may be demonstrated, however, by other methods of stimulation than the cutting of nerves. The stimulation of nerve trunks and especially of nerve centers in bony-fishes by an induction-current is followed by a lightening of the fish, that is, by a concentration of its melanophore pigment. If the medulla of a killifish is exposed and stimulated electrically, the fish becomes rapidly light (Mills, 1932).

If a catfish in whose tail-fin a dark band has been produced is stimulated on the roof of the mouth by an electric current, the whole fish becomes light except the dark caudal band (unpublished observation by A. A. Abramowitz). Thus the assumption of the light phase in these fishes is associated with nerve activation; in short, concentrating fibers must be admitted to be present.

Another kind of evidence bearing on this question is shown in the responses of denervated faded bands and of normal light bands to pairs of flanking dark bands. When two flanking dark bands are excited on either side of a denervated faded band, the faded band after a short interval darkens as already described (Fig. 3). If instead of a denervated band the equivalent of such a band entirely normal in that it is fully innervated is flanked by two dark bands, the inclosed light band remains light and never darkens (Fig. 4). The only difference between these two flanked bands is that the one that darkened was denervated and the other that remained light was normally innervated. I know of no way by which this difference can be explained except by assuming that innervation in some way protects the flanked band from the invading darkening neurohumor of its surroundings. Such protection naturally implies the presence of concentrating nerve-fibers whose action would be opposed to that of the invading neurohumor, and hence I conclude that concentrating fibers must be present. Such fibers are more freely open to electrical than to mechanical stimulation the latter being especially effective for dispersing fibers (Mills, 1932). Thus the evidence for concentrating fibers though not so clear as that for dispersing fibers is nevertheless sufficiently strong to justify the conclusion that these fibers are present in bony-fishes as well as in dogfishes.

Do concentrating nerve-fibers act on melanophores through appropriate neurohumors? To this question a final answer cannot be given. So far as observations go, however, there is no evidence that such a humor may not be present, and such evidence as there is favors its existence. This evidence

is seen in the way in which the caudal bands are lost. If the disappearance of a caudal band in a killifish or in a catfish, where the band is larger and stronger (Figs. 5 to 8), is carefully watched, it will be found to take place from the sides of the band inward toward its axis (Figs. 12, 13, 14). The edge of the band is at first sharp, the line of separation between the melanophores with fully concentrated and those with fully dispersed pigment being very clear (Fig. 12). This sharpness is gradually reduced (Fig. 13) and finally almost entirely disappears (Fig. 14). The band, however, does not vanish as a whole, but, as has just been indicated, it seems to wear away on its edges till it is reduced to a narrow streak after which it finally fades completely. If the band disappeared in consequence of the cessation of nerve activity, it should fade more or less uniformly throughout its whole width in accordance with the spread of the dispersing fibers. The same would be true if its disappearance was due to the slow washing out of the effective neurohumor by the body fluids, for these fluids bathe the whole under surface of the band contained in the skin. The fact, however, that the band fades on its edges first and that this process proceeds from the edges toward the axis argues in favor of a lateral invasion by some agent. The lateral areas surrounding the band are the light areas of the tail-fin whose melanophore pigment is in the concentrated state. So far as I can see the only means by which these light areas can influence the dark band is through a transferred neurohumor which of course must be a concentrating one. I, therefore, regard the method of disappearance of dark bands as evidence in favor of concentrating neurohumors. I admit, however, that this evidence is by no means final.

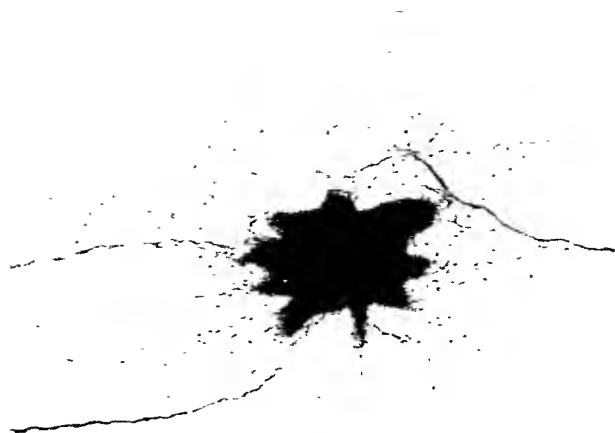
The reactions of the melanophores in the two bony-fishes here considered, the catfish, *Ameiurus nebulosus*, and the killifish, *Fundulus heteroclitus*, point indubitably to the double innervation of these effectors in that both dispersing fibers (parasympathetic) and concentrating fibers (sympathetic) are clearly present. It is also equally certain that the

dispersing fibers act through a dispersing neurohumor which is apparently not soluble in blood but which may be so in lipoids. Whether there is a concentrating neurohumor or not is by no means so clear, but such evidence as there is on this question is of an affirmative kind. The established presence in bony-fishes of a dispersing neurohumor and the probable presence of one for concentration are results favorable to the neurohumoral hypothesis.

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PLATE I



1



2

FIG. 1. Innervation of a melanophore from a percid. After Bellow et al. 1893.

FIG. 2. Tail-fin of *Fundulus* showing a capillary glass-tube (A) as a solid block and the dark bands resulting from cutting nerves proximal and distal to the object. The proximal cut was made first and the band produced by it did not extend beyond the block.

For the preparation of the photographs on plates I to XI I am indebted to Dr. F. M. Carpenter. In Figures 2, 3, and 4 the tinted areas were added in pencil.

PLATE II



3



4

FIG. 3. Tail-fin of *Amelurus* in which a faded band is partly flanked by two new dark bands. The faded band darkened between the two flanking bands, but not in the part proximal to the flanking bands.

FIG. 4. Tail-fin of *Amelurus* in which a normal unpigmented area is flanked by two new dark bands. The normal area remained light.



5

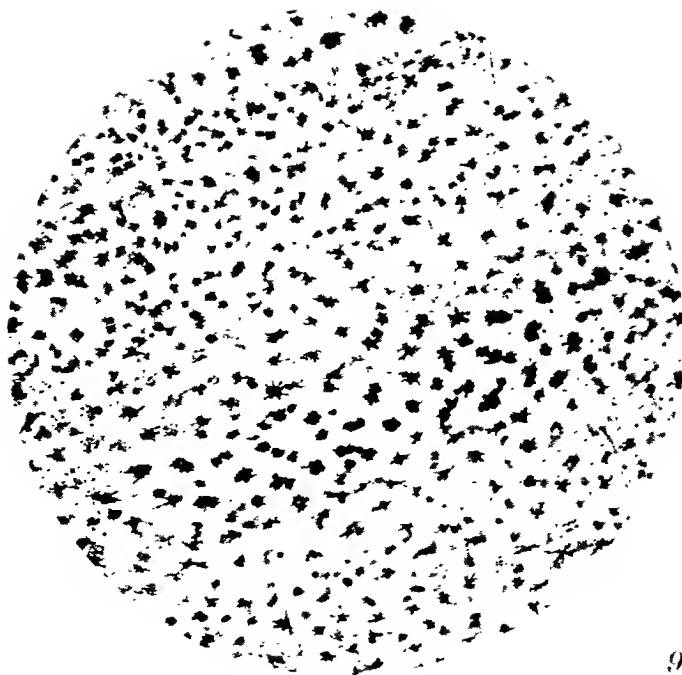


6

FIGS. 5 AND 6. Tail-fins of *Ameiurus* showing the formation and disappearance of a band; Fig. 5, a newly formed band; Fig. 6, a slightly faded band



FIGS. 7 AND 8. Fossils of *Ammonoites* showing the petiole and disappearance of
 petiole from the blade in the fossil and the petiole in the Fig. 7 or Fig. 8. The
 last places in the text.



9

FIG. 9. Melanophores with concentrated pigment from the light part of the flanked faded band in Fig. 3.



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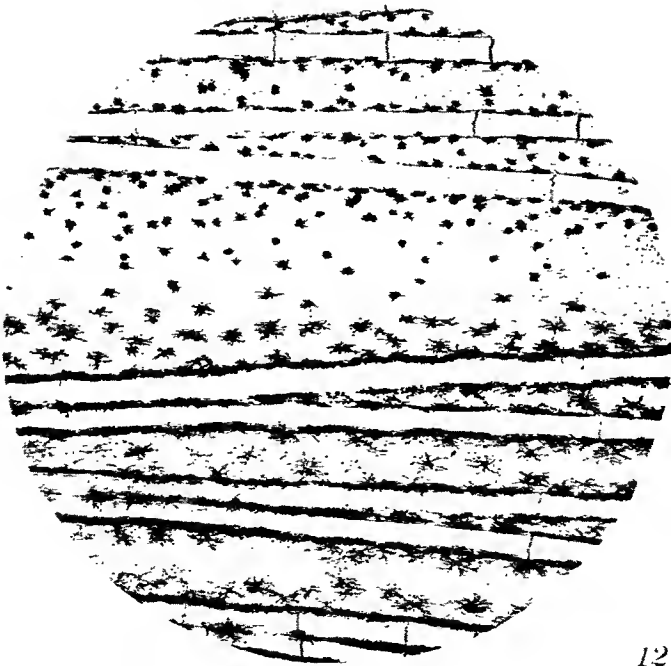
FIG. 10. Melanophores with partly dispersed pigment from the dark part of the flanked band in Fig. 3.

FIGS. 9 TO 11. States of melanophores from three regions shown in Fig. 3, tail-fin of *Anchoa*.



11

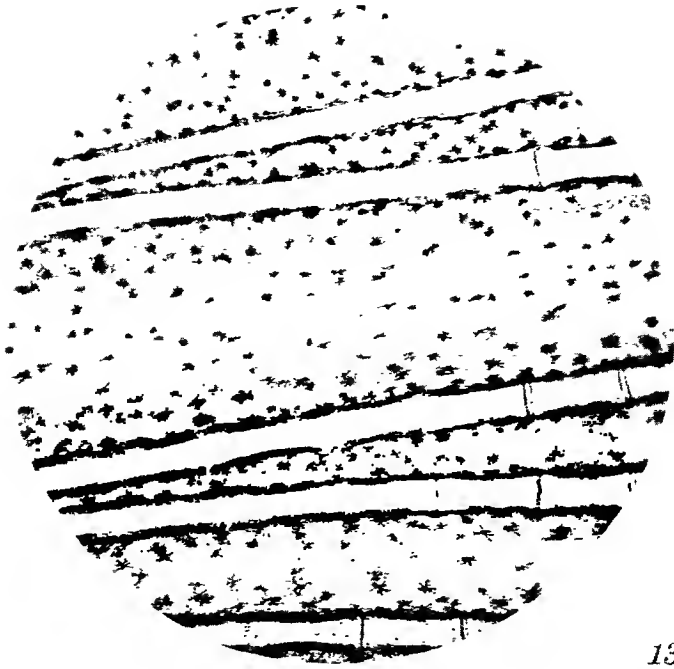
FIG. 11. Melanophores with fully dispersed pigment from one of the flanking dark bands in Fig. 5.



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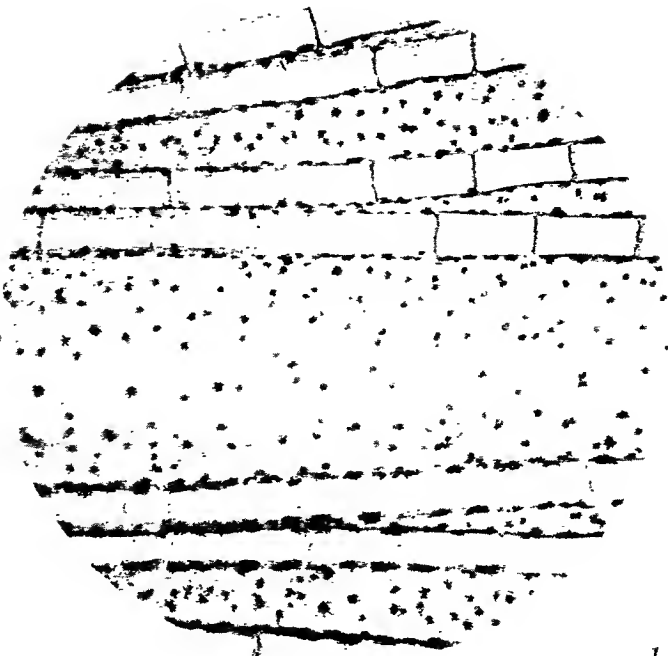
FIG. 12. Edge of a newly formed band with melanophores with dispersed pigment (darker area) in the lower part of the photograph and those with concentrated pigment (general light area of film) in the upper part. The separation between these two areas is sharp.

FIGS. 12 TO 14. States of melanophores at the edge of a band in the tail-fin of *Tribolodon*.



13

FIG. 13. The edge of a band similar to that shown in Fig. 12 but some hours after its formation; the lower part of the figure is toward the band-melanophores with dispersed pigment, and the upper part toward the light area of the fin. The sharp line between these two areas is largely lost.



14

FIG. 14. Edge of a band similar to that shown in Fig. 12 but a day or so after its formation; orientation as in Figs. 12 and 13. The sharpness of the edge is entirely lost.

NEW LIGHT ON PREHISTORIC MAN IN ASIA

GEORGE GRANT MACCURDY

(Read April 21, 1934)

Russia.—During the fifty years prior to 1921, only about twenty Paleolithic sites were discovered in all Russia. These earlier discoveries were confined largely to Russia in Europe and to the region of Krasnoyarsk in the Yenisei valley (Siberia). Since 1921, seventy additional Paleolithic and several dozen Mesolithic stations have been reported. The Crimea and Caucasus have proved to be exceptionally rich fields, especially as regards caves and rock shelters. During the past ten years, many important sites have been explored in this region, which through its geographic position and climatic conditions, is more or less closely linked with Africa and southern Europe as a whole. Mention should be made of Bonch-Osmolovsky's excavations at Kiik-Koba in the Crimea and those in the valleys of the Dnieper and the Don.

Passing now to Siberia, noteworthy are the finds at a site in the open, on the Bjelaja river near Malta, some 60 km. west of Irkutsk. This station was discovered by Gerassimov in 1928 and excavated the following year. The many art objects are all of bone, patinated and weathered to a grey-brown color. The objects include figures in the round of the bird and of the human female, incised plaques, pendants and beads. One perforated plaque has a punctate pattern on one side and a linear serpentine pattern on the other. Another object with punctate pattern may represent the head of a snake.

Of the six bird figurines, one is at rest; the other five are posed as if in flight: long outstretched neck and stubby wings spread, but so short as to be scarcely differentiated from the body and tail. The station of Mezin (Russia in

Europe) has likewise yielded bird figurines. The most remarkable series from Malta includes twenty figurines of the human female. All, with certain variations due in part to the material employed, are in the same class with the female figurines from France, Italy, Austria, Germany, Czechoslovakia and Russia in Europe. Three have perforations between the feet instead of at the head end, differing in this respect from the stylistic figurines found at Petersfels, Germany. In some cases, the hair is indicated, in others, it is not. All undoubtedly symbolize fecundity—a mother cult. Four female human figurines of bone were recently found at Kostenki and five of ivory at Gagarino, both sites being in the valley of the Don. Among the Paleolithic treasures from Malta, is the skeleton of a child with a necklace and a circlet of ivory on the skull.

China.—During the past ten years, important discoveries have been made in the Caves of Choukoutien, some 64 km. southwest of Peiping. The principal finds have come to light since 1927: ramus of an adult human lower jaw (1928), adult human cranium (1929), cranial cap (1930), and stone artifacts of quartz and rock crystal. Teilhard de Chardin would refer the human and fossil animal remains to the early Pleistocene; he ascribes the industrial remains to the Lower Paleolithic. Breuil does not give the industry a definite date, but considers that its author was not inferior in mentality to the Neandertalians of Corrèze and Dordogne. The race, now known as *Sinanthropus pekinensis*, had a certain facility in chipping stone implements and knew how to make use of fire.

During the season of 1933, there have been found in the upper cave of Choukoutien skeletal and industrial remains of a race, that differs both physically and culturally from the one found in the main cave. Mr. Pei thinks the upper cave represents a late Paleolithic (Magdalenian) culture; this is probably true, since the finds include a finely chipped flint implement and a bone needle. The bones of the skull found with these are thinner than are those from the lower cave.

In this connection, it will be well to add a brief summary of the available data on the prehistoric sequence in North China. The Sanmenian (Late Pliocene) beds have thus far yielded no trace of human occupation. The Choukoutien *Sinanthropus* culture is the oldest known in China and is referred to the Lower Pleistocene. The Upper Pleistocene deposits of North China have furnished data as follows: (1) Quartzite implements of Mousterian type are found in the basal gravels of the true loess in eastern Kansu, northern Shansi and western Shansi; a strongly rolled quartzite implement was found in the basal gravels of the Hoangho, near Poote; and a workshop of paleolithic age has been reported from the uppermost part of the basal gravels near Wupao. (2) The superposed loess itself contains isolated artifacts of quartz in eastern Kansu and near Poote. (3) Camp and workshop sites occur in the Pleistocene deposits along the Shuitungkou, as well as the Sjara-osso-gol (southeastern Ordos).

The loess deposits of Shuitungkou are 20 m. thick and, at several levels, contain many artifacts of a Mousterio-Aurignacian type: blade and flake industry, such as scrapers, scratchers, points and gravers, in association with remains of *Rhinoceros tichorhinus*, *Hyæna*, etc. The Upper Pleistocene deposits along the Sjara-osso-gol are 80 m. thick, with a single cultural (microlithic) layer some 60 m. deep. With these microliths are found abundant remains of *Hyæna spelæa*, *Equus hemionus*, *Bos*, *Rhinoceros tichorhinus*, *Elaphus*, *Bubalus*, *Elephas namadicus*, etc. The Sjara-osso-gol and the Shuitungkou cultures are believed to be contemporary; they represent the Upper Paleolithic of northern China. No traces of the Paleolithic have as yet been found south of the Tsingling Mountains in central and southern China. The Neolithic is found abundantly all over the Manchurian-Mongolian area in a rather thin layer of black superficial earth.

Java.—In September, 1931, Dr. Oppenoorth, the Dutch geologist, discovered human skeletal remains in a mid-

Pleistocene deposit on the banks of the Solo river, Java, only about 10 km. from the spot where Dubois found *Pithecanthropus* in 1891. The cranium is almost complete with the exception of the basal portion and the face bones. It has been christened *Homo soloensis* and is of a higher type than *Sinanthropus*, the latter being about midway between *Homo soloensis* and *Pithecanthropus*. According to Sir Arthur Keith, we thus have in that part of the Old World an evolutionary series as follows: (1) *Pithecanthropus*, (2) *Sinanthropus*, (3) *Homo soloensis* and (4) the living primitive Australian.

Palestine.—One year ago, I reported on the results of the fifth season of excavations in the Wady al-Mughara, near Athlit, Palestine, conducted jointly by the American School of Prehistoric Research and the British School of Archaeology in Jerusalem. They were confined largely to the Tabun cave (Mugharet et-Tabun), which has turned out to be the largest cave of the Wady al-Mughara group. The work of the sixth season revealed that the deposit is very thick (Pl. I). The fifth (counting from the top) layer alone is 5 m. in thickness and may be subdivided into four distinct levels, all of Acheulio-Mousterian age. Below this thick Layer E are two more: Layer F (1.90 m. thick) and Layer G (2 m. thick). Layer F is true Acheulian, according to Miss Garrod. Layer G contains a crude industry derived from the Clactonian and called by Breuil Tayacian, which is older than the Acheulian and may date as far back as the Mindel-Riss Interglacial Epoch. The Tayacian of Layer G at Tabun probably corresponds in age with the beginning of the Riss-Würm Interglacial. Layers F and G in the Tabun cave are comparable in age with the two culture-bearing layers found by Neuville in the Umm Qatafa cave some 15 km. south of Jerusalem.

A composite section of the three caves in the Wady al-Mughara consists of a total of sixteen culture levels, some of which are repeated in all three caves. The combined thickness, not counting the repetitions, amounts to more than

21 m. The thickness of the Tabun cave deposits alone is 15 m. This composite section from the Tayacian to the topmost layer represents a period of more than 100,000 years (Pl. II).

Even more important than the rich harvest of artifacts from the various layers of this composite section are the human skeletal remains, chief among them being at least ten individuals of the Neandertal race and several dozen of the Natufian (Mesolithic) race. The Neandertal skeletons are now being chiseled from their stony matrixes. The work is being done in the laboratories of the Royal College of Surgeons, London, and is in charge of Theodore D. McCown of the American School, who is on leave of absence from the University of California, for this purpose. The work is under the able supervision of Sir Arthur Keith. Nine of the skeletons were found by McCown in the Skhul cave and one was found by Miss Garrod in the Tabun cave. The cost of preparing these priceless specimens for publication is being borne equally by the Royal College of Surgeons and the American School of Prehistoric Research. The American Council of Learned Societies has made a generous grant toward covering the School's share of the expense. The School is also much indebted to the University of California for granting to McCown a fellowship and the necessary leave of absence. Thus far, two tons of plaster and cement have been removed from two of the Skhul skeletons by means of a pneumatic chisel. The Tabun skeleton is now in the final stages of cleaning.

The skeleton from the Tabun cave is that of an adult female. The limbs are partially extended, in contrast with the flexed burials from the near-by Skhul cave. The supra-orbital torus is very pronounced for a female. This feature and the tremendous inter-orbital breadth are reminiscent of both the Galilee skull and the Skhul specimens. The vaulting of the frontal and the size of the cranium (Pl. III) seems to be less than in the crania from Skhul. The lower jaw has been repaired; the condyles are missing (Pls. IVa and Va).

It is relatively slender; the ascending rami slope backward and there is a total absence of chin. In all these respects, it differs from the massive lower jaw from a somewhat greater depth in the same Layer at Tabun (Pls. IV*b* and V*b*). The latter is in a class with the lower jaws from Skhul.

The limb bones of the skeleton from Tabun, in comparison with those of the Skhul skeletons, are relatively short and slight (Pl. VI). The long suspected, but hitherto undiscovered, vertebral column of skeleton No. IV from Skhul is coming to light. The carpus and finger bones of both hands of this individual are nearly complete, as is the tarsus and the toe bones of the right foot. These parts will add much to our knowledge of the physical characters of Neandertal man.

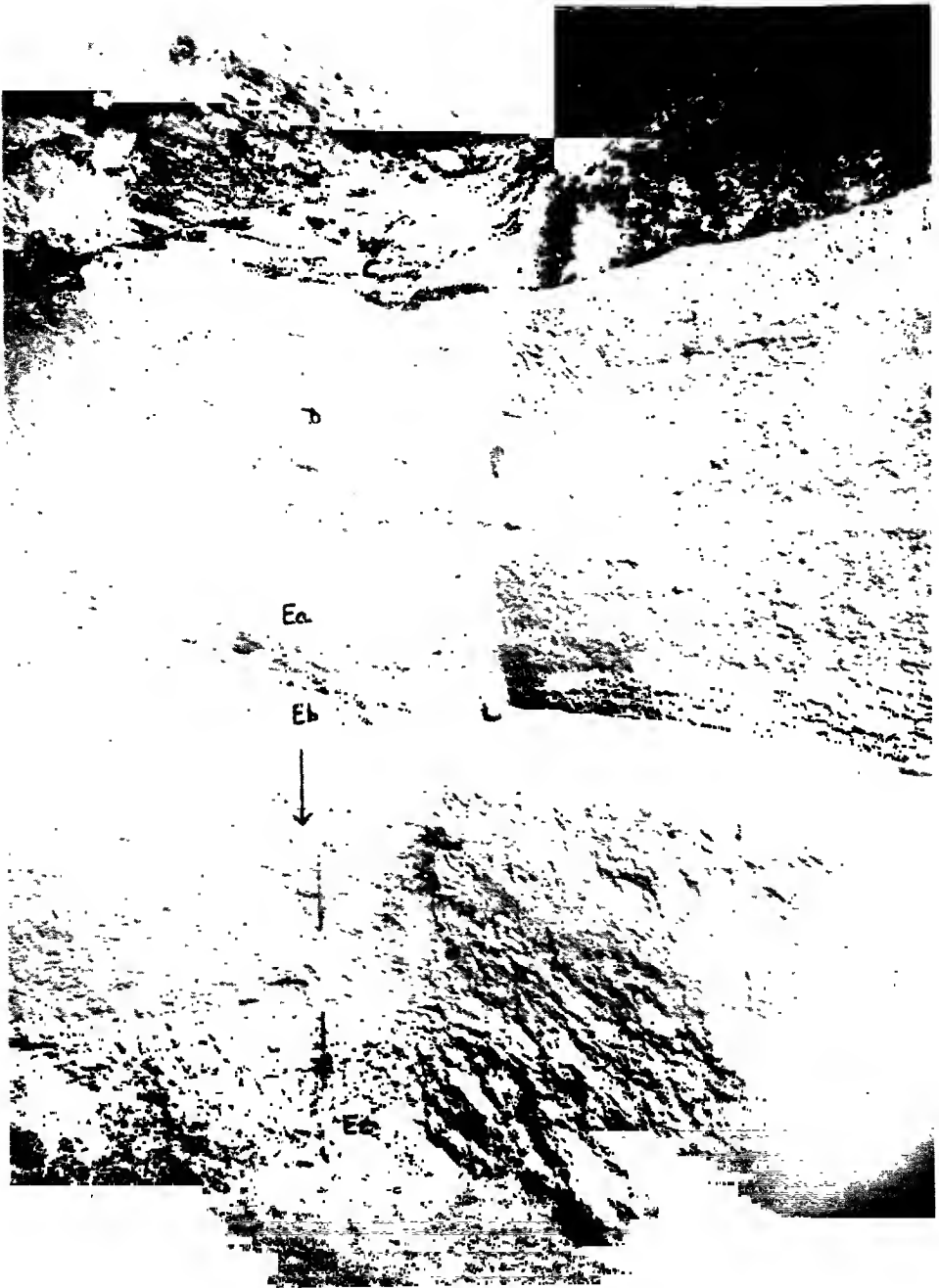
Miss Garrod found the middle part of the diaphysis of a human femur at Tabun in Layer Ea; this is much older than Layer C, which yielded the chinless low-browed and slightly built female and the massive lower jaw. This femur fragment, which dates from the Acheulio-Mousterian, has no *linea aspera* and is not platymeric (*i.e.*, the anteroposterior diameter of the shaft is considerably less than the transverse diameter).

For three months of the present year, beginning on February 1, a temporary exhibition of the results of the joint Palestine Expeditions was installed at the British Museum, Bloomsbury, through the courtesy of the Trustees of the Museum and the auspicious coöperation of Reginald Smith, Keeper of the Department of British and Medieval Antiquities, and T. D. Kendrick and Christopher Hawkes, Assistant Keepers. Miss Garrod and Mr. McCown arranged the exhibition so as to include: (1) a complete sequence of the cultural remains from the Tayacian to the Natufian; (2) various animal bones from the different layers; and (3) some of the Neandertal and Mesolithic (Natufian) skeletons.

The seventh season of joint excavations by the two Schools is now in progress at the Tabun cave with Miss Garrod in charge. It is proposed to complete the excavations

by the first of July, any remaining portion of the deposit being left as a control section for future reference. These joint excavations have already proved that Palestine is of prime importance, not only as a geographic and historic, but also as a prehistoric link in the chain, which binds together the three continents of the Old World.

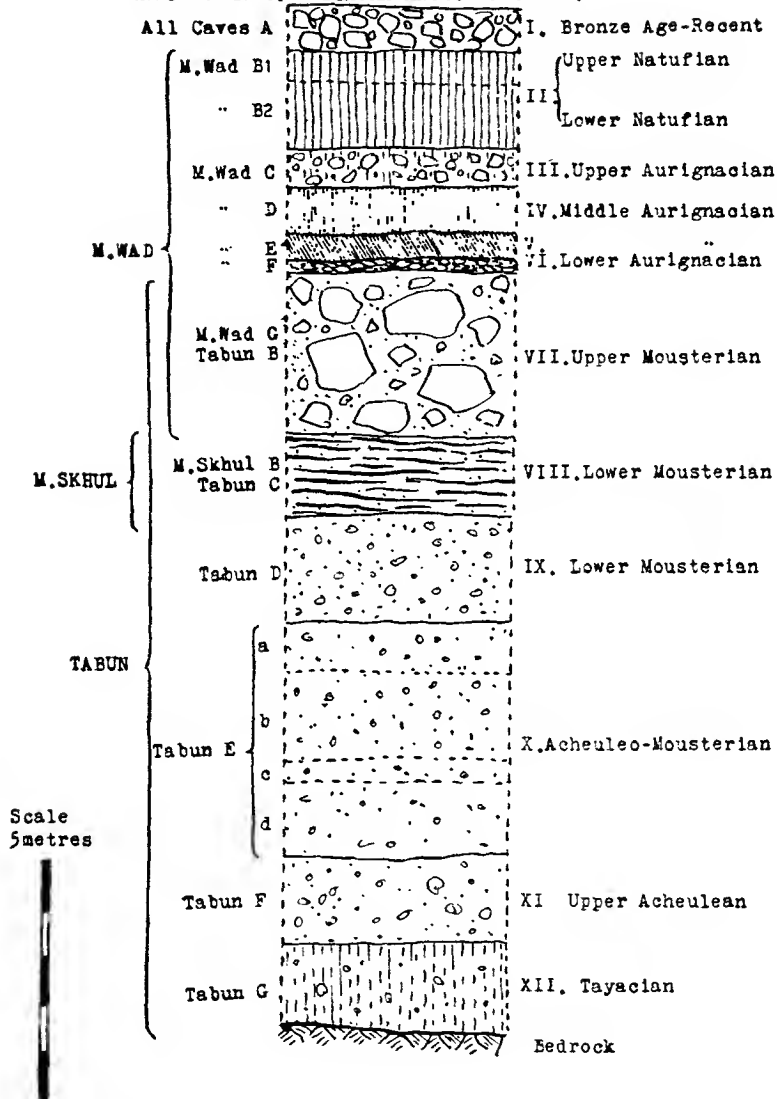
PLATE I



Mirdas et-Tabun (Cave of the Oven). All the layers are visible except: A (Bronze Age to Recent) at the top and Ea (Acheulo-Moustérien) and F and G (Acheulén and Tayacien) at the bottom.

PLATE II

WADY MUGHARA. DIAGRAMMATIC SECTION.



Composite section of the three Wady al-Mughara caves; it represents a period of over 100,000 years and its thickness, not counting duplications of layers, is more than 21 meters. Prepared by Miss Garrod.

PLATE III



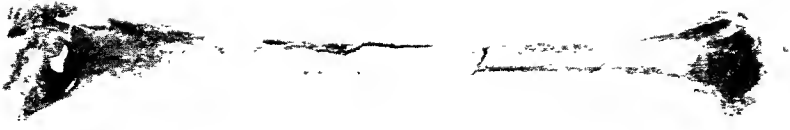
Frontal bone of the cranium from Layer C. 1. For a view of a normal human skull.

PLATE IV



(a) Chinless lower jaw of the skeleton from Layer C, Tabun cave; (b) lower jaw from Layer C, Tabun cave, resembling those found in the near-by cave of Skhul. Normal laterals.

PLATE VI



a



b



c

Left upper limb of a human skeleton. The bones are from the same individual as the ones in Plate V. The bones are from the same individual as the ones in Plate V.

PROCHROMOSOMES AND CHROMOSOME STRUCTURE IN *IMPATIENS* *

FRANK H. SMITH

Read by title, April 21, 1934

INTRODUCTION

THERE are many controversial questions regarding prochromosomes. The structure of nuclei which show prochromosomes, the nature of prochromosomes, their origin and their relation to the continuity of chromosomes have all been variously interpreted. Various names have been given to the chromatic structures found in the nuclei of certain plants but the term "prochromosome" suggested by Overton (16) probably has the widest usage.

Rosenberg (18) was the first to show that the number of prochromosomes is approximately the same as the number of chromosomes. Prochromosomes have been considered as structures identical with chromosomes (Malte, 13; Stout, 28; Eichhorn, 2), as centers about which chromosomes form during the prophases (Laibach, 9; Overton, 17; Rosenberg, 20) and as portions of chromosomes which persist through the telophases (Heitz, 5; Grégoire, 3; Doutreligne, 1). It has also been considered that prochromosomes bear no direct relation to chromosomes (Lundegårdh, 12; Tischler, 30) or that they are not directly concerned in the formation of prophase chromosomes (Kuhn, 8). De Smet (24) stated that prochromosomes may not be present in meristematic nuclei.

Another point which has been variously interpreted is that of the pairing of prochromosomes in somatic and premeiotic nuclei. Overton (17) stated that they are all paired while Rosenberg (19) and Laibach (9) stated that only some

*Papers from the department of Botany and Herbarium of the University of Michigan, no. 464.

are paired. Heitz (6), Grégoire (3) and Doutreligne (1), however, concluded that the prochromosomes are not paired in somatic nuclei. Several authors have described a complete fusion of prochromosomes in resting and meristematic nuclei.

There has been much discussion regarding the structure of nuclei which exhibit prochromosomes. Laibach (9), Doutreligne (1) and others have stated that the prochromosomes are located peripherally in a nucleus which is otherwise homogeneous. There is therefore no reticulum such as appears in nuclei which do not show prochromosomes. Rosenberg (18), Schiller (21) and Kuhn (8) found that a fibrous network rarely occurs while De Smet (24), Lundegårdh (12) and Zacharias (32) described a reticulum similar to that found in other nuclei. Recent work on chromosome structure in plants with large chromosomes has shown that the chromonemata persist as fibers in the resting nucleus. Grégoire (3) and Doutreligne (1) stated that in *Impatiens Balsamina* there are no fibers in the resting nucleus and that the fully formed chromosomes do not show chromonemata. Thus the species which show prochromosomes are sharply set apart from other species with regard to these structures, which are generally considered to represent the cytological basis for the interpretation of genetical behavior.

The reduction divisions in species with prochromosomes have not been studied as extensively as the somatic divisions. Several authors have described a sequence which is radically different from that which is characteristic of the majority of species in which the reduction divisions have been studied. This is especially true for the stages prior to synizesis. Rosenberg (19) and Lundegårdh (11) stated that the prochromosomes are usually paired in the pre-meiotic nuclei. Overton (17) considered that the prochromosomes are paired at the earliest stages of the heterotypic division and possibly in the preceding telophases. The early spireme then consists of two univalent strands which lie parallel in the nucleus. De Souza (27) stated that there is no leptotene stage in *Impatiens Balsamina* but that the single strands which

appear in the nuclei prior to synizesis are bivalent. Thus the prochromosomes are considered to have paired completely before and during the formation of these strands. In most species it is considered that the pairing of homologous chromosomes does not occur until approximately the time of the synizetic contraction.

MATERIALS AND METHODS

The present study is based on material of *Impatiens Balsamina* L. (*Balsamina hortensis* DC.) and the Camellia variety of this species. A study of the two varieties showed that they are identical in their chromosome number and nuclear structure. Root tips were used for the somatic divisions and comparisons were made with the somatic and pre-meiotic nuclei in the flower buds.

Various fixatives were used, including Benda's, Allen's modification of Bouin's, Showalter's, Flemming's medium and strong solutions, and Navashin's as modified by Sax and by Karpechenko. The two latter solutions gave the best results for root tips. Flemming's medium and Sax's solutions proved the most satisfactory for the meiotic divisions. The perianth was removed from the older flowers and all buds were dipped into a Carnoy's solution for a few seconds before being immersed in the fixing fluid. The usual paraffin method was used and sections were cut from 4 to 15 microns in thickness.

Various stains were used, including Heidenhain's iron-alum hæmatoxylin, brazilin, Flemming's triple stain and a modification of the gentian-violet-iodine stain. With the latter stain the slide was washed quickly with a concentrated solution of picric acid in absolute alcohol during the later stages of dehydration after the staining. This treatment leaves the chromatic material a dark purple and the rest of the nucleus and the cytoplasm a light, transparent yellow. This stain was the most satisfactory of those used.

Other species of *Impatiens* were studied to determine the presence or absence of prochromosomes and also the chromosome numbers.

OBSERVATIONS

Somatic Divisions

There are fourteen somatic chromosomes in *I. Balsamina* as has previously been reported. Two of these chromosomes bear satellites (Pl. I, Fig. 1). All of the chromosomes have either median or sub-median spindle-fiber constrictions. The chromosomes are strongly chromatic at the equatorial-plate stage and no internal structure could be differentiated with any of the stains used.

The anaphase chromosomes are somewhat elongated but still strongly chromatic. Sometimes they present a moniliform outline which has been interpreted as an indication of the presence of an internal spiral. Rarely a spiral could be identified in anaphase chromosomes (Fig. 2). Because of the compactness of the chromosomes at this stage it could not be determined whether this spiral represents a single chromonema or two closely associated chromonemata. Subsequent stages indicate the latter alternative.

At the conclusion of the anaphases the chromosomes group rather closely at the poles of the spindle. Each chromosome soon appears to become somewhat flattened. The telophasic changes first become evident at the extremities of the chromosomes. The matrix disappears and achromatic strands, two from each chromosome, extend to the nuclear membrane (Fig. 3). It is evident at this stage that the chromosomes are double in nature. These strands are considered to correspond to the two chromonemata which have been described during this and earlier stages in plants with larger chromosomes (Kaufmann, 7; Sharp, 22; Teleżyński, 29). In *Impatiens* the two strands of each chromosome tend to diverge during the early telophases.

As the telophases progress the chromosomes separate somewhat and there is a gradual reduction in the amount of chromatic material. At this stage the chromosomes are noticeably double, and connecting fibers are evident between the various chromosomes and also between the half-chromo-

somes (Fig. 4). The chromonemata remain chromatic for some distance on either side of the point of spindle-fiber attachment.

During the later telophases there is an apparent increase in the amount of chromatic material in the regions adjacent to the points of attachment and a decrease in other regions of the chromosomes (Fig. 5). The nucleolus is fully formed at this stage. Approximately at this time the portions of chromosomes definitely assume a peripheral position in the nucleus, which is maintained until late in the succeeding prophase. Two heavy fibers, usually divergent, extend from each end of each prochromosome. The fibers represent the chromonemata of the chromosomes. A few additional fibers are present. There is a further reduction in the amount of chromatic material adjacent to the point of attachment during the late telophases.

Thus in the interphase nucleus the fourteen prochromosomes are comparatively small, peripherally located in the nucleus and connected by a network of fibers, at least some of which represent chromonemata (Fig. 6). The prochromosomes may vary in size and shape but the point of spindle-fiber attachment is usually evident. As Heitz (6) observed, the satellites remain chromatic and may be attached to the nucleolus. This is also true in nuclei of older tissues in which divisions are rare (Figs. 18, 19). Thus the nucleolus is not centrally located in the nucleus but always lies close to the peripheral reticulum.

In the meristematic interphase nuclei many of the prochromosomes show indications of a double nature. This was also shown by Heitz (6) and by Doutreligne (1). This doubleness has been shown in the present study to result from the double nature of the early telophase chromosomes. The full diploid number of prochromosomes appears in most of the meristematic nuclei. No evidence could be found of any tendency toward pairing or fusion. The shape of the prochromosomes is by no means constant in the meristematic nuclei. Some of the variations are illustrated in figures 7

and 8. These differences in structure result from the loss of varying amounts of chromatic material and the varying divergence of the two chromonemata of the same chromosome. Thus the prochromosome may be represented by two small chromatic masses as shown in figure 7. The two masses appear side by side but this is not to be interpreted as the pairing of two prochromosomes. The point of spindle-fiber attachment lies between these masses and the two represent only one chromosome.

Definite spirals are found in some meristematic nuclei (Fig. 7). These appear to result from the close association of the two chromonemata of a chromosome during the preceding telophases. Apparently in this case much of the chromatic material is then retained on the chromonemata. This interpretation is strengthened by the appearance of the prochromosome at the top of the nucleus in figure 8. Here the chromonemata at the right of the point of attachment are closely associated and appear as a single spiral. The chromonemata are divergent on the left side which presents the usual appearance of the prochromosome at this stage. The author has previously suggested that the chromatic knots in the resting nuclei of *Galtonia* (25) result from the persistence of chromatic material at points where the two chromonemata of a single chromosome are in contact. The same explanation might be applied here since usually the two chromonemata of a chromosome diverge greatly and are separate at all points except at that of the spindle-fiber attachment.

As the prophases are initiated the prochromosomes become more chromatic and chromatic material begins to collect on the fibers which extend from the ends of each prochromosome (Fig. 9). As the fibers become more chromatic apparently there is a progressive reassociation of the two chromonemata of each chromosome (Fig. 10). Thus the prochromosomes become longer and more slender but they always appear double at the ends. In the later stages the filament assumes a somewhat spiral form (Fig. 11). Some connecting fibers remain but these are extremely fine and lightly stained.

During the middle prophase the connecting fibers gradually disappear and only the fibers remain which represent the chromonemata (Fig. 12). These fibers are usually double and lie more or less parallel. They may appear somewhat granular. As they become larger and more chromatic they come together and appear as a single strand (Fig. 13). During these stages and shortly afterward it is possible to obtain a differential staining of the chromonemata and their surrounding matrix. The spiral appears single at first but soon becomes double (Fig. 14). With the further accumulation of a matrix around each half chromosome the usual late prophase chromosomes are formed (Fig. 15). These are noticeably double and, with further contraction, form the metaphase chromosomes.

Apparently in some cases the prophases follow a more simple sequence. Figure 16 illustrates the type of early prophase nucleus which sometimes occurs. This type was also figured by Heitz (6). There are practically no fibers present except those which represent chromonemata. During the prophases these fibers each acquire a matrix, contract, and thus form almost directly the late prophase chromosomes. Figure 17 illustrates late prophase chromosomes which were probably formed in this manner. The two halves may remain divergent on the equatorial plate.

In nuclei in the region of elongation and in older cells the prochromosomes vary as to size, shape and number. At least one (Fig. 18) and sometimes both (Fig. 19) of the satellites are in contact with the nucleolus. Some of the prochromosomes are so small that they can hardly be identified. It might be assumed then, that in those nuclei in which less than fourteen prochromosomes are found there are some prochromosomes which have entirely lost their capacity to take stain. There is no evidence of pairing or fusion of the prochromosomes in these nuclei. There is a fine reticulum which consists in part of fibers representing chromonemata. This reticulum is not evident with such fixatives as Benda's and Carnoy's solutions. The net does not show clearly with

hæmatoxylin unless a counter-stain is used. It shows most clearly in sections which have been over-stained with gentian-violet.

Meiotic Divisions

The pre-meiotic nuclear divisions are essentially similar to the somatic divisions in the root tips. Figure 20 (Pl. II) illustrates a complete nucleus in a late telophase stage of the last pre-meiotic division. A comparison with figure 6 (Pl. I) shows that the nucleus at this time has a structure similar to that of a meristematic interphase nucleus. There is no evidence of a pairing of the prochromosomes and the full diploid number is present. As the nucleus enlarges the prochromosomes may become somewhat smaller, and in some cases, lose entirely their capacity to take stain. Thus the occurrence of less than the diploid number does not result from a pairing or fusion of prochromosomes.

The first indication of the initiation of the heterotypic prophases is a gradual accumulation of slightly chromatic material around the fibers which extend from the ends of the prochromosomes (Pl. II, Fig. 21). Generally the full diploid number of prochromosomes is evident at this time and there is still no evidence of pairing. This stage apparently corresponds to that illustrated in figure 10 (Pl. I) of the somatic divisions. In both cases there is a thickening of the pair of fibers which extends from the ends of each prochromosome.

As the chromatic material on these fibers increases, the two fibers become so closely associated as to appear as a single strand (Pl. II, Fig. 22). Thus figure 22 is apparently comparable to the somatic prophase stage shown in figure 11 (Pl. I). In both there is the progressive reassociation of the two fibers of each chromosome to form an apparently single strand. In these stages it is sometimes impossible to identify all fourteen prochromosomes because of the irregular accumulation of chromatic material.

As the prophases progress all finer fibers disappear and the leptotene strands remain free in the nucleus. Figure 23 (Pl. II) illustrates three leptotene strands at this stage.

These strands are univalent as the preceding stages clearly indicate. During the later leptotene stages the strands may become evenly chromatic so that the prochromosome regions and the points of spindle-fiber attachment cannot be identified.

The evidence thus far indicates that the leptotene stage does exist in *I. Balsamina*. Overton (17) described the early heterotypic prophases of *Thalictrum* as consisting of two parallel spiremes. Lundegårdh (11) found that although the prochromosomes may be arranged side by side during the early prophases they do not conjugate until the time of synizetic contraction. De Souza (27) stated that univalent leptotene strands do not exist in *I. Balsamina*. Leliveld (10) concluded that prochromosomes tend to pair in the earliest prophases but that some univalent strands are present after synizesis.

Shortly before the initiation of the synizetic contraction the leptotene strands begin to pair side by side (Fig. 24). Apparently the portions of homologous strands adjacent to the spindle-fiber attachments tend to pair first but this could not be shown definitely. As the strands become paired they also become more chromatic. Thus the regions which represent the prochromosomes are no longer recognizable in these double strands. Consequently the number of chromatic bodies present in the nucleus becomes less and less as the pairing progresses. This probably accounts for the variable number of prochromosomes described by various authors during synizesis. As the strands open out after the synizetic contraction no chromatic bodies are evident but the bivalent strands are evenly chromatic.

The spindle-fiber constrictions again become evident in the late pachytene strands (Fig. 25). No internal structure could be differentiated in the strands until the initiation of the separation of homologous chromosomes. At this stage a single spiral is occasionally found before the two strands separate (Fig. 26). One of the pairs illustrated in this figure is the pair of satellite chromosomes which is shown attached to the nucleolus.

During diakinesis the chromosomes shorten and thicken and there is an increase in the amount of achromatic material in the nucleus in the form of fibers and granules (Fig. 27). Chromonemata have been found in some of the middle diakinesis chromosomes such as the pair illustrated in figure 28. Only portions of the spiral in each chromosome appear double. The chromosomes do not exist as tetrads either at this stage or on the equatorial plate (Fig. 29). The internal structure of some of the metaphase chromosomes is shown in figure 30. Usually the spiral in each chromosome at this stage is single throughout all or most of its length. During the early anaphases each chromosome becomes double (Fig. 31). There is little matrix present at this time. During the late anaphases a single spiral can sometimes be differentiated (Fig. 32).

At the conclusion of the anaphases the chromosomes group rather loosely at the poles. The individual chromosomes can easily be identified in the early telophases (Fig. 33). Few connecting fibers are present and the chromonemata have lost little of their enclosing matrix. As the telophases progress the chromonemata gradually lose their chromaticity, the loss proceeding from the ends toward the point of spindle-fiber attachment (Fig. 34). Small nucleoli appear in the chromosomes at this time. These stain red with Flemming's triple stain whereas the chromosomes stain blue. The number of nucleoli is variable. Some chromosomes show no nucleoli, some one and others two. Apparently the matrix of the chromosome is continuous with the body of the nucleolus as McClintock (cited by Sharp, 23) found in corn.

Seven prochromosomes are visible in the interkinetic nucleus. Usually the nuclei pass almost directly from the stage illustrated in figure 34 into the prophases of the homoeotypic division. During these stages the nucleoli enlarge somewhat but retain their positions in the chromosomes (Fig. 35). The strands in the nucleus which represent chromonemata become more chromatic and the long prophase chromosomes are formed.

During the middle and late prophase the nucleoli gradually disappear (Fig. 36) from all the chromosomes except the satellite chromosome. In the later stages all fibers and nucleoli disappear and normal late prophase chromosomes are formed. The homoeotypic metaphase (Fig. 37) and anaphase chromosomes are small and densely chromatic. No internal structure could be differentiated in the homoeotypic chromosomes during these stages.

The telophases of the homoeotypic divisions are essentially similar to those of the somatic divisions with the exception that in the homoeotypic telophases only seven prochromosomes are present (Fig. 38). Each prochromosome shows indications of a doubleness at its ends. During the cytokinesis of the pollen mother cell there is a considerable increase in the size of the nuclei. Coincident with this increase in size is a change in the appearance of each nucleus. Many of the fibers become somewhat chromatic so that the prochromosomes are no longer sharply defined (Fig. 39). The seven prochromosomes can usually but not always be identified. The satellite chromosome is recognizable, attached to a large nucleolus. Smaller nucleoli are in contact with other chromosomes. The ensuing prophase of the first division of the microspore are similar to those of the somatic divisions in the root tips.

DISCUSSION

The various interpretations in the literature of the nature and significance of prochromosomes have been briefly mentioned in the introduction to this paper. These interpretations will now be discussed with regard to the present study.

The first question to be considered is that of the nature of the prochromosomes themselves. The author's observations agree with those of Heitz (6), Grégoire (3) and Dautreigne (1). That is to say, a prochromosome represents that portion of a chromosome on either side of and adjacent to the point of spindle-fiber attachment. The spindle-fiber constriction is usually recognizable in the prochromosome. This region generally remains chromatic throughout the

somatic chromosome cycle. Thus the prochromosome is derived directly from the early telophase chromosome by the persistence of the chromatic material in the region of the spindle-fiber attachment. In the nuclei in older tissues, and less commonly in meristematic tissues, the region on either side of the constriction may become achromatic. This results in the apparent disappearance of that particular prochromosome. Thus all nuclei do not exhibit the full diploid number of prochromosomes.

This variation in number may be associated with the descriptions of fusion of prochromosomes in resting nuclei. The evidence obtained in this study suggests that such fusions do not occur in normal resting or meristematic nuclei. It seems more probable that the disappearance of some prochromosomes, and the consequent occurrence of less than the diploid number, is to be attributed to the complete loss in chromaticity of the portion of the chromosome adjacent to the point of spindle-fiber attachment. It has also been considered that pairing of prochromosomes might account for the appearance of less than the diploid number in both somatic and pre-meiotic nuclei. It is true that some prochromosomes appear to be composed of paired chromatic bodies (Pl. I, Figs. 6, 7, 16). A study of the cycle in somatic nuclei clearly indicates that this doubleness is a result of a previous division and not of a pairing of two separate prochromosomes.

Another point which has been variously interpreted is the presence or absence of a reticulum or fibers in the nucleus in addition to the prochromosomes. It is probable that the different fixatives and stains used account for the various contradictory descriptions. Nebel (14) has shown that the presence of fibers between chromonemata in a resting nucleus may result from inadequate fixation. It is to be expected, however, that chromonemata would be present in some form. Fibers may be found in nuclei which show prochromosomes with Flemming's medium, Navashin's, various modifications of this formula, Bouin's and other solutions. Fibers are

found rarely or not at all with Carnoy's, Benda's and Flemming's strong solutions. Regardless of the fixative used, it is difficult to demonstrate the presence of achromatic fibers with hæmatoxylin without the use of a counter-stain. Fibers are stained readily with Flemming's triple stain, the Gram stain contrasted with picric acid, or with other dyes which stain achromatic material. In the present study fibers were a constant feature of the resting and meristematic nuclei and at least some of these fibers represent chromonemata.

The presence of fibers in the nucleus is thus directly associated with the presence or absence of chromonemata in the chromosome cycle. Grégoire (3) and Doutreligne (1) stated that chromonemata are not present in plants with prochromosomes. This is a very important consideration in view of the present conception of the cytological structure of chromosomes and nuclei in its relation to genetical behavior. It is difficult to differentiate the internal structure in these small chromosomes so the evidence obtained in this study is somewhat incomplete. It does, however, strongly suggest that the chromosome cycles in both somatic and meiotic divisions are essentially similar to those of plants with large chromosomes. So far as is known, plants with prochromosomes behave genetically like those without such structures. It appears from this study that the fundamental structures of chromosomes and nuclei in *I. Balsamina* are essentially similar to those of species without prochromosomes and with larger chromosomes.

This species also appears to be similar to other species with regard to the time of pairing of homologous chromosomes in the heterotypic prophases. No evidence was found to support the description given by De Souza (27) of a conjugate pairing of the prochromosomes in *I. Balsamina*. In the present study it is concluded that the formation of univalent leptotene strands is essentially similar to that of the single strands in the early prophases of the somatic divisions. These leptotene strands may show prochromosomes and spindle-fiber constrictions until they pair side by side prior to and during the synizetic contraction.

OTHER SPECIES OF *Impatiens*

Very little is known regarding chromosome numbers in the genus *Impatiens*. There are over 200 species and these occur chiefly in Africa and India. The chromosome number of *I. Balsamina* has been reported at various times. Ottley (15) reported the chromosome number of *I. Sultani* as probably 7 pairs. Winge (31) gave 20 as the number of somatic chromosomes in *I. noli-tangere*. Heitz (4, 5) reported 20 somatic chromosomes in *I. parviflora*, 16 in *I. Holstii* and 14 in *I. Mathilda*. In the present study the occurrence of prochromosomes and the chromosome numbers were determined for various species of *Impatiens*. Most of the counts were made only from the meiotic divisions.

I. scabrida DC. (*I. tricornis* Lindl.) has seven pairs of chromosomes which appear to be somewhat larger than those of *I. Balsamina* (Pl. III, Fig. 40). The points of spindle-fiber attachment, however, are in the same relative positions (Fig. 41). There is one pair of satellite chromosomes. The resting and meristematic nuclei do not show prochromosomes.

I. Sultani Hook. f. has eight pairs of chromosomes. This was determined from somatic metaphases (Fig. 42), late diakinesis (Fig. 43) and homoeotypic metaphases (Fig. 44). There is one pair of satellite chromosomes. Heitz (5) observed that the nuclei do not show typical prochromosomes.

I. Holstii Engl. & Warb. (*I. Walleriana* Hook. f.) also has eight pairs of chromosomes (Fig. 45). This number was previously reported by Heitz (5). These chromosomes appear to be identical in form to those of *I. Sultani* when examined on the homoeotypic plate (Fig. 46). Prochromosomes do not appear in the nuclei.

I. Roylei Walp. (*I. glanduligera* Lindl.) has nine pairs of chromosomes, one of which is somewhat larger than those of *I. Balsamina* whereas the rest are smaller (Fig. 47). This difference in size is more readily observed in the homoeotypic metaphase chromosomes (Fig. 48). All of the smaller chromosomes show median spindle-fiber constrictions and the larger chromosome has a sub-median constriction. Prochromo-

somes are present in the resting nuclei. *I. Roylei* var. *alba* has chromosomes of the same size and shape as *I. Roylei*. Prochromosomes are also evident.

I. bi flora Walt. (*I. fulva* Nutt.) has ten pairs of chromosomes, two pairs of which bear satellites (Fig. 49). The homoeotypic metaphase chromosomes are all approximately of equal size and show median spindle-fiber constrictions (Fig. 50). Thus they resemble more closely the smaller chromosomes of *I. Roylei* than those of *I. Balsamina*. Prochromosomes occur in the resting nuclei.

I. pallida Nutt. (*I. aurca* Muhl.) also has ten pairs of chromosomes including two pairs which bear satellites (Fig. 51). The homoeotypic chromosomes are very similar to those of *I. bi flora* in both size and points of spindle-fiber attachment (Fig. 52). The reduction divisions in this species exhibit spindles which are extremely curved about the periphery of the pollen mother cell (Smith, 26). Prochromosomes appear in the resting nuclei. All four satellites are evident in the somatic nuclei. The number of satellites which are in contact with the nucleolus is variable. In some cases only one (Fig. 53) and in other cases more than one are in contact.

In the species of *Impatiens* which have been studied there is a sequence of 7, 8, 9 and 10 chromosome pairs. This probably does not represent a simple aneuploid series, however, since the individual chromosomes vary in size and point of spindle-fiber attachment in the various species. The occurrence of prochromosomes bears no relation to the number of chromosomes. Additional work is necessary before any conclusions can be drawn regarding the relations of the species in this genus.

SUMMARY

1. The cycle of the chromosomes and prochromosomes has been followed in both the somatic and meiotic divisions of *Impatiens Balsamina*.
2. The prochromosomes in somatic nuclei are derived directly from the chromosomes by the persistence of chromatic

material on either side of and adjacent to the points of spindle-fiber attachment.

3. There is no pairing or fusion of prochromosomes in either somatic or pre-meiotic nuclei.

4. Fibers are regularly present in resting and meristematic nuclei. At least some of these fibers represent chromonemata.

5. Chromonemata have been observed in the chromosomes of both somatic and meiotic divisions.

6. Evidence is submitted which strongly suggests that the chromosome structure as well as the chromosome cycle in plants with prochromosomes are fundamentally the same as in plants without prochromosomes and with larger chromosomes in both somatic and meiotic divisions.

7. There is a sequence of 7, 8, 9 and 10 pairs of chromosomes in the species of *Impatiens* which have been studied. There is no relation between chromosome number and the occurrence of prochromosomes.

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EXPLANATION OF PLATES

The drawings were made with the aid of a camera lucida at a magnification of approximately 4500 diameters. They have not been reduced in reproduction. For clearness, many of the drawings represent only portions of nuclei and are indicated as such.

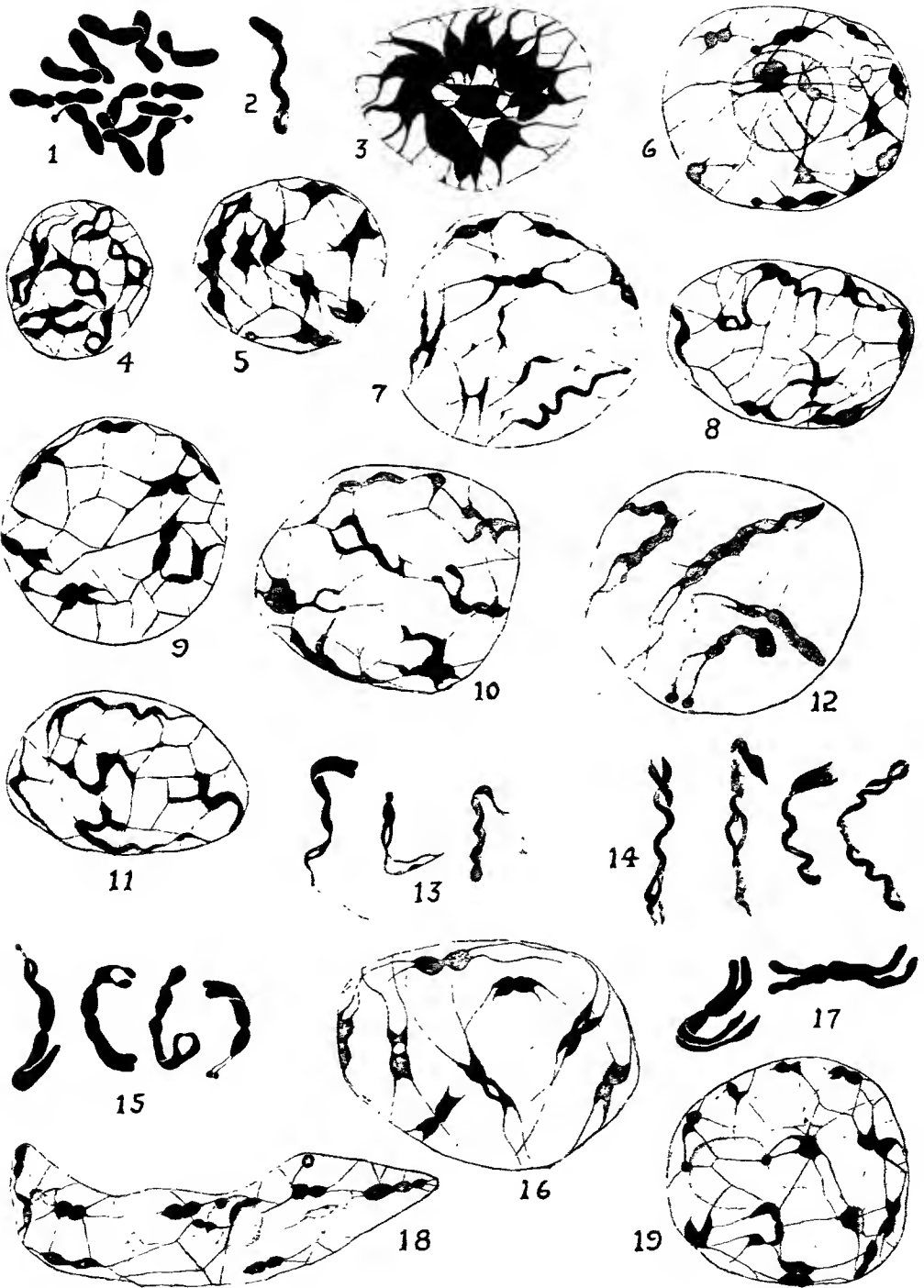
Figures 1-19 show nuclei or chromosomes from the root tips of *I. Balsamina* and figures 20-39 show stages in the mitotic divisions of the same species. Figures 40-53 show chromosomes of various other species of *Impatiens*.

PLATE I

Senary Division of I. Balsamina

- FIG. 1. Metaphase plate as seen in polar view.
 FIG. 2. An anaphase chromosome which shows an internal spiral structure imbedded in a small amount of matrix.
 FIG. 3. Polar view of an early telophase stage showing the initial reduction of chromatic material at the ends of the chromosomes which are double at this time.
 FIG. 4. A portion of an early telophase nucleus showing the two chromonemata of each chromosome.
 FIG. 5. A portion of a middle-late telophase nucleus showing the prochromosomes which are double with two diverging fibers extending from the ends of each.
 FIG. 6. An entire nucleus from the meristematic region. One of the satellites is in contact with the nucleolus. Fourteen prochromosomes are present.
 FIG. 7. A portion of a meristematic nucleus showing the presence of spirals and also apparently paired prochromosomes.
 FIG. 8. The same, showing other variations in shape of the prochromosomes.
 FIG. 9. A portion of an early prophase nucleus showing the increase in chromaticity of the fibers extending from the ends of each prochromosome.
 FIG. 10. A portion of a later prophase nucleus showing the progressive reassociation of the divergent halves of each chromosome.
 FIG. 11. The same at a slightly later stage.
 FIG. 12. Three chromosomes from a middle prophase nucleus after most of the connecting fibers have disappeared.
 FIG. 13. A later stage in which the fibers which represent chromonemata have become chromatic.
 FIG. 14. Middle and late prophase chromosomes in which an internal structure has been differentiated.
 FIG. 15. Late prophase chromosomes.
 FIG. 16. A portion of an early prophase nucleus in which few fibers appear except those which represent chromonemata.
 FIG. 17. Late prophase chromosomes which were probably derived from prochromosomes such as those shown in fig. 16.
 FIG. 18. Resting nucleus from the region of elongation. One satellite is in contact with the nucleolus.
 FIG. 19. Same, from the cortex. Both satellites are in contact with the nucleolus.

PLATE I



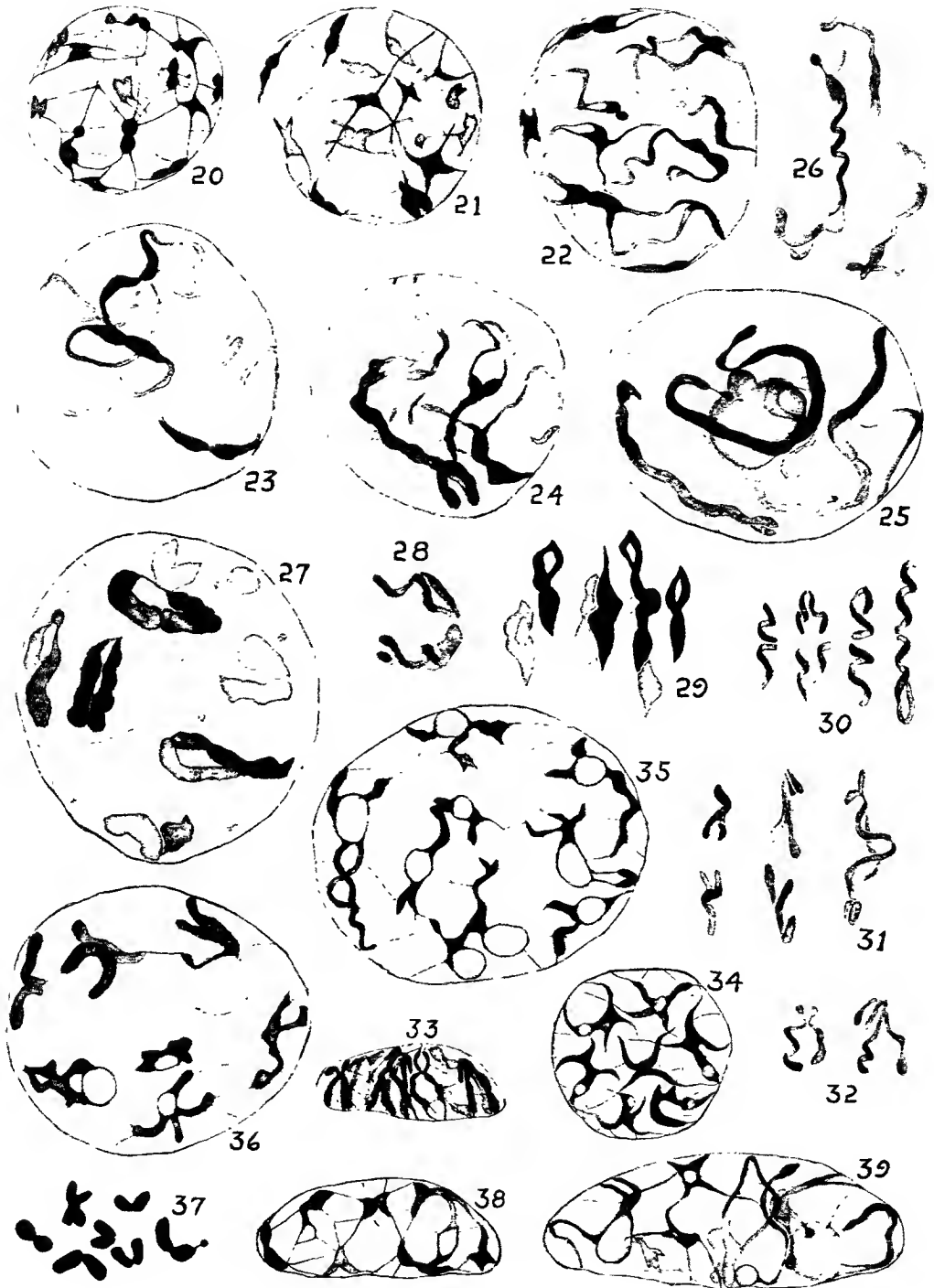
SOMATIC DIVISIONS OF *I. Balsamina*

PLATE II

Meiotic Divisions of I. Balanina

- FIG. 20. A nucleus in a late telophase stage of the last pre-meiotic division.
- FIG. 21. A pre-leptotene nucleus showing the fourteen unpaired prochromosomes, and the increase in chromaticity of the fibers which represent chromonemata.
- FIG. 22. A portion of a late pre-leptotene nucleus showing the progressive reassociation of the halves of each chromosome.
- FIG. 23. Univalent leptotene strands showing the prochromosome regions and the spindle-fiber constrictions.
- FIG. 24. The initiation of pairing of the leptotene strands at the beginning of synizetic contraction.
- FIG. 25. Late pachytene stage. The pair of satellite chromosomes is shown attached to the nucleolus.
- FIG. 26. The separation of the chromosomes at late pachytene.
- FIG. 27. Middle diakinesis.
- FIG. 28. A chromosome pair at middle diakinesis showing chromonemata.
- FIG. 29. Heterotypic metaphase.
- FIG. 30. Chromonemata in metaphase chromosomes.
- FIG. 31. Early anaphase chromosomes.
- FIG. 32. Late anaphase chromosomes showing chromonemata.
- FIG. 33. Early telophase stage of the heterotypic division.
- FIG. 34. Polar view of a slightly later stage showing nucleoli.
- FIG. 35. A polar view of an early prophase stage of a homoeotypic division.
- FIG. 36. Same of a later stage. The satellite chromosome is in contact with the largest nucleolus.
- FIG. 37. Homoeotypic metaphase.
- FIG. 38. Middle telophase nucleus of the homoeotypic division.
- FIG. 39. Nucleus of a young microspore.

PLATE II



MEIOTIC DIVISIONS OF *I. Balsamina*

PLATE III

- FIG. 40. *I. scabrada*. Diakinesis.
FIG. 41. Same, homœotypic metaphase.
FIG. 42. *I. Sulzani*. Somatic metaphase.
FIG. 43. Same, late diakinesis.
FIG. 44. Same, homœotypic metaphase.
FIG. 45. *I. Holsii*. Late diakinesis.
FIG. 46. Same, homœotypic metaphase.
FIG. 47. *I. Roylei*. Heterotypic metaphase.
FIG. 48. Same, homœotypic metaphase.
FIG. 49. *I. biflora*. Diakinesis.
FIG. 50. Same, homœotypic metaphase.
FIG. 51. *I. pallida*. Late diakinesis.
FIG. 52. Same, late homœotypic prophase.
FIG. 53. Same, early prophase nucleus of a tapetal cell. The nuclear network has been omitted.

PLATE III



CHROMOSOMES OF VARIOUS OTHER SPECIES OF *Impatiens*

SOME ASPECTS OF GENETIC CONSTITUTION IN RELATION TO PATHOLOGY

JOHN W. GOWEN

(Read April 21, 1934)

A MORBID process may be affected by the genetic constitution of an animal in 4 general ways: (a) Genes normal to a species may, by mutation, cause physiological and developmental processes so abnormal that death or lasting disability results. Death of the homozygous yellow mice in the progeny of the yellow parents (1) or the disabilities of *Drosophila* affected by the focal melanosis gene as seen in Fig. 1 are examples of this type (2). (b) The segregation of specific genes for susceptibility or resistance to disease caused by pathogens may be responsible for immunity, morbidity or mortality within the exposed population. The liver disease of mice due to *B. piliformis* almost universally attacks one species, the Japanese waltzers (3, 4), the common mouse being resistant. The crosses between the two species are, in general, resistant. The second generation backcrosses and F_2 's segregate in a manner suggesting a single Mendelian factor difference as the cause of the inheritance of susceptibility and resistance. (c) An unbalance in the proportions of genes making up the body cells may cause death or a greatly reduced length of life (5). In *Drosophila* there are four types of chromosomal organization; males with one sex-chromosome and two sets of autosomes, females with 2 sex-chromosomes and 2 sets of autosomes, triploid females with 3 sex-chromosomes and 3 sets of autosomes and intersexes with 2 sex-chromosomes and 3 sets of autosomes. The ratio of the sex-chromosomes and their contained genes to the autosomes for the four types would be males $X/2A$, females $2X/2A$, triploid females $3X/3A$ and intersexes $2X/3A$. The females and triploid females are balanced, the males have an

unbalance but one established through long selection, the intersexes are unbalanced. The survival value of these different types is in direct relation to the balance, the females and triploid females have the greatest length of life, the males are shorter lived, and the intersexes show a still shorter length of life. (d) During development the body, due to its inheritance, may have a mosaic of cells, some normal, some abnormal, and the morbidity and mortality increase directly with the proportion of defective cells in the mosaic (6). In *Drosophila* there are inherited variations which result in adult flies whose cells form a mosaic of normal and abnormal genetic constitutions. The number of abnormal cells which are formed is controlled by the temperature at which the organism is kept during the first 3 days of life. When the proportion of abnormal to normal cells becomes large in flies subjected to low temperature, the males all die and the number of females is reduced nearly to half that of the normal controls. Furthermore, the surviving females are debilitated, sterile in many cases, and live but a short time. If now we reverse the picture by keeping the flies at a higher temperature throughout their life, the mottling is greatly reduced; the males live and breed and the females are essentially normal. It is thus evident that a mosaic of abnormal vs. normal cells in an organism tends to hinder vital functions and that when this condition is extreme, death follows.

In view of the pronounced effects which the genes have on the immunity, morbidity and mortality of a disease, the question immediately follows, how many genes make up the genetic constitution of the organism and what proportion of them are absolutely essential to life? It can be shown that the numbers of genes making up the genetic constitution of an organism may be more than ten thousand and that at least 90 per cent are so vital that experimental alteration of any one of them causes death. Favorable material for such a study is now practically limited to one animal form, *Drosophila*. The chromosomes and their contained genes make up an essential part of each cell. They are in pairs; one of each



FIG. 1. Effects of focal melanosis gene. On left, normal male and female above; below, male and two females showing black melanotic lesions of the femur-tibia junction of the legs. On right, paper at bottom of culture bottle showing 62 flies dead with focal melanosis.

pair is derived from the male parent through the sperm, the other from the female parent through the egg. Particular genes have designated locations, loci, within the chromosomes. A photomicrograph showing the somatic chromosomes of a *Drosophila* female just prior to mitosis is shown in Fig. 2. These chromosomes are in pairs, two chromosomes of each kind; a pair of medium length, the sex or first chromosomes, 2 pairs of long V-shaped chromosomes, the second and third, and a pair of small chromosomes, the fourth. Besides these, this female also has a Y-chromosome. The total length of one of each pair of these chromosomes I, II, III, IV, not including the Y-chromosome, is 6.85×10^{-4} cm., the breadth and thickness is 0.33×10^{-4} cm. The individual chromosomes have corresponding sizes.

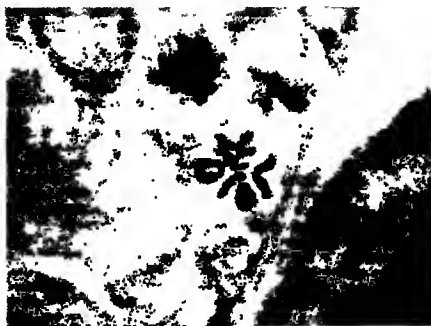


FIG. 2. Photomicrograph of the chromosomes of *Drosophila* showing diploid group plus an extra Y. Magnification 2500.

The third figure shows the chromosomes as they are condensed into the sperm head. They apparently enter it end for end since the total length of the sperm head is 7.36×10^{-4} cm. and the breadth 0.37×10^{-4} cm., the total length and breadth including beside the chromatin, the acrosome, cytoplasmic sheath, etc. The genes which are contained in these chromosomes are ordinarily relatively stable structures. The fact, as Muller originally showed (7), that these genes can be qualitatively altered by X-rays offers us the same possibilities of studying gene structure that similar techniques

have presented to the physical sciences in the study of crystal or atomic structure. From data obtained by this method it is possible to estimate roughly the kind, number and size of genes. If we think of the X-rays as being absorbed by the chromatin as discrete units absorbed at random within the sperm head, the target, we may estimate the ratio of gene material to the total chromatin as the ratio of the number of the discrete units which cause mutation to the numbers which are absorbed in all of the chromatin. The

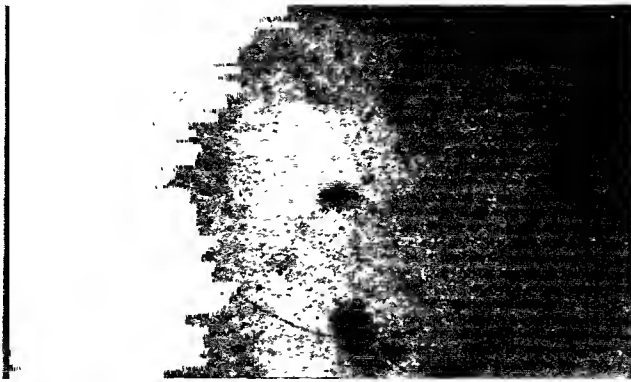


FIG. 3. Sperm of *Drosophila*. The tail of this sperm is at least three times as long as shown, and quite possibly longer since it may have been broken in handling the material. The acrosome, cytoplasmic sheath, etc. of the sperm head is stained in this preparation. Magnification 770.

numbers of the different kinds of genes may be estimated from the observed different mutations. The upper limit of the mean size of an average gene may be derived by dividing the number of the different kinds of genes into the amount of gene chromatin. It is evident from the photograph of chromosomes in Fig. 2 that the amount of chromatin within any one or all of the chromosomes may be measured, provided we know the magnification.

The types of survivorship curves which we get when we expose the chromosomes to monochromatic X-rays from copper or chromium targets is shown in Fig. 4. If we interpret the deaths as being due to dominant lethal mutations which

would be capable of killing the organism even though they were transmitted by only one parent, we may obtain an estimate of the numbers of such genes.

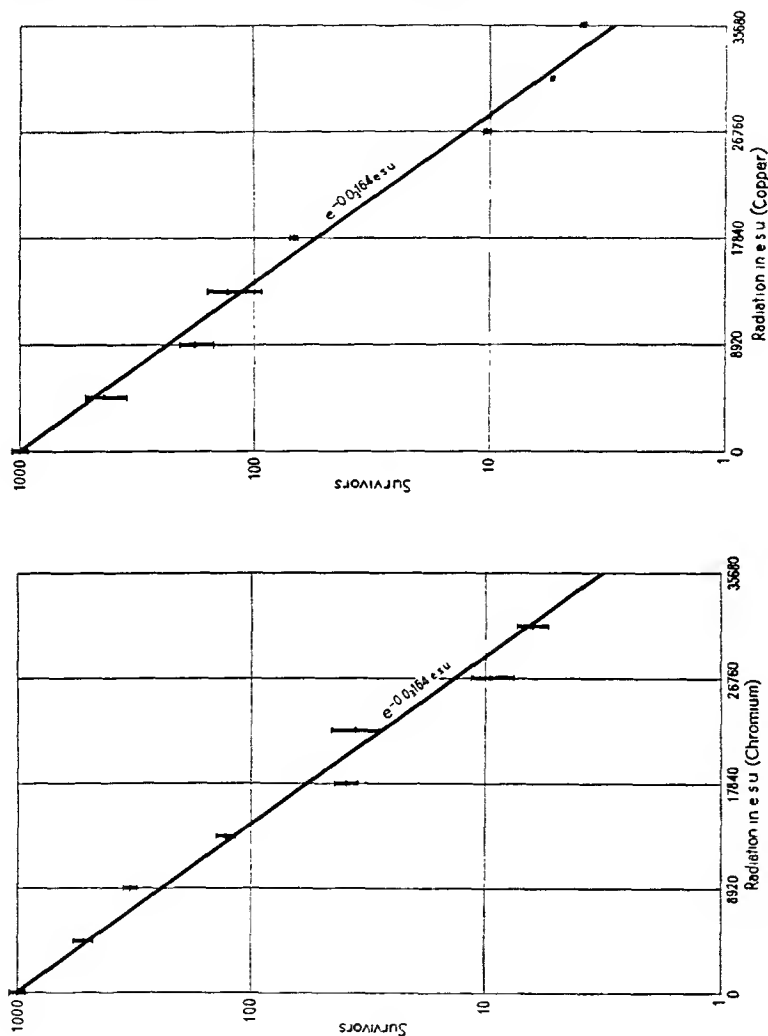


FIG. 4. Survivorship curves of *Drosophila* exposed to X-ray.

The survival ratios on semi-logarithmic grid form a straight line of constant slope without any lag period. It follows that this type of curve would be generated on the

condition that one absorption in a vital gene would alter it so that death would result. The equation expressing this condition is

$$S = e^{-ant},$$

where a is the probability of one absorption within the vital gene, n the number of absorptions in unit time and t is time of exposure. The number of absorptions by the chromatin may be calculated from a knowledge of the type of X-rays used in these experiments. If sperm were entirely composed of vital recessive genes then every absorption would be expected to kill. If only a tenth of the chromatin was gene material then only a tenth of the absorptions would strike vital genes and cause death. The observed hits shown on the chart are much less than this value, in one experiment the theoretical absorptions are 1.163 per second while the effective absorptions are 0.024 per second, a ratio of 1 : .021. In other words, the normal sperm has 21 thousandths of its volume composed of this type of gene.

The data on the ratio of males to females with respect to

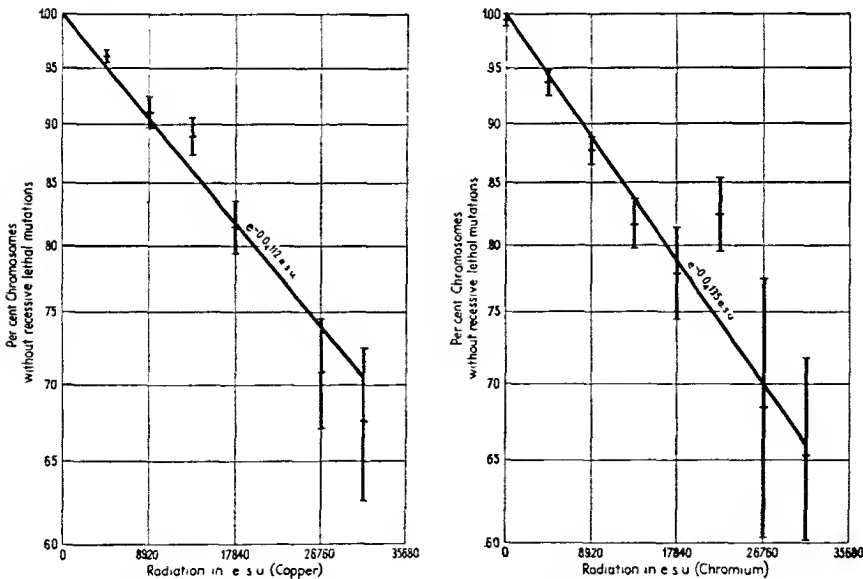


FIG. 5. Rate of recessive sex-linked lethal mutations on exposure to X-ray.

survivorship furnish further information on the rates of mutation of the dominant lethals, since the experiment is so arranged that such lethals can be transmitted only to females. These lethals depress the proportion of surviving females to males, the resulting curves being essentially like those of the previous figure.

A third type of gene transmutation by X-rays is the recessive lethal where the gene kills only when it is homozygous, as in the case of the yellow mice of Cuénot. Curves showing the sex-linked lethals produced in the sex chromosome are seen in Fig. 5. Here again the type of the curve remains the same. From the ratios of these different kinds of gene changes, one with the other, we may obtain the proportions of the different kinds of genes.

For our purposes, one of the most significant considerations is that of the viability of animals containing the gene transmuted by the X-rays. Of the 302 genes with sufficient progeny to determine their viability, 225 or about 75 per cent were completely lethal. Only 3 per cent approached the viability of the wild type genes from which they transmuted. The distribution of these genes based on their survival value is shown in Fig. 6.

Out of the total of 364 genes observed in the sex-chromosome, 44 produced visible effects quite comparable with those seen in so-called natural mutations, and had a viability of 20 per cent or more. A comparison of our data with those of Morgan, Bridges and Sturtevant, derived from a study of natural mutations, showed that 6 of the 44 genes were identical with theirs and 8 others were probably so. The criteria for the latter were similarity in morphological effects and location within the chromosome. The genes considered in Morgan, Bridges and Sturtevant's data were contained in 42 loci within the sex-chromosome (8). The chance of having a gene in one of their 42 loci would be equal to $42/\text{total loci}$. If we consider our 44 genes as a unique sample of this population the chance of drawing one or more of these loci would be $(42/\text{total loci}) \times 44$, which in our case equaled either 6

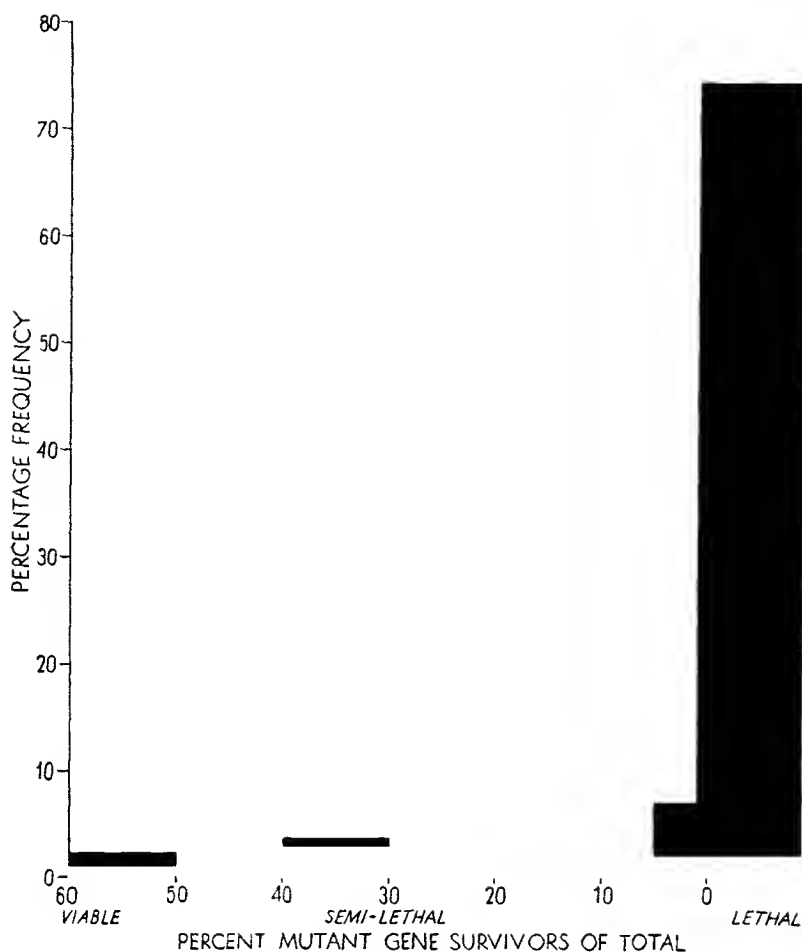


FIG. 6. Frequency distribution showing the sample of genes and the proportions of them which are completely lethal, which produce pathogenic effects making them semi-lethal, and are fully viable. The ordinates represent percentage of genes in given class of the total. The abscissa gives the percentage of survivors in the gene mutant class divided by the whole number of survivors. 0 = completely lethal; 50 = normal viability.

or 14, depending on whether the proven allelomorphous genes or likely allelomorphous genes were utilized. This gives a total of 132 or 308 loci for the sex-linked genes having morphological effects and a viability of at least 20 per cent. There are several other ways of estimating the gene loci in the sex-

chromosome leading to results which are within this range. We shall adopt provisionally 175 to represent these gene loci.

With this number of genes causing a visible morphological effect as a basis for calculation, it follows from the data just presented that the sex-linked recessive lethal genes would be equal to $320'44$ or $7.3 \times 175 = 1280$ loci. The dominant mutations producing visible effects had a frequency comparable with that of similar sex-linked mutations = 175. The rate at which the sex-linked recessives mutations were produced was 0.04123, that for the dominant lethal mutations in the sex chromosome was 0.0392 a ratio of 1.0 : 0.75 or 960 loci. The rate at which mutations are produced in the sex-chromosome was 0.00557 that for the autosomes was 0.0175, or since the chances of producing a mutation with any one gene in the autosomes or in the sex-chromosomes may be considered equal, it would follow that there is only about $1/3$ as much gene material in the sex chromosome as in the autosomes. From this relation we find the number of genes capable of mutating to dominant lethals to be $960/0.098$ or 9800 and of recessive lethal genes to be 13100. The number of visible recessive genes becomes 1925. The estimates of the proportion of different kinds of genes in the genetic constitution are listed in Table I.

TABLE I

Type of Loci	Number	Per Cent
Sex-linked loci of visible factors.....	175	0.6
Sex-linked loci of recessive lethal factors.....	1280	4.7
Sex-linked loci of dominant lethal factors.....	960	3.5
Autosomal loci of visible recessive factors.....	1800	6.6
Autosomal loci of recessive lethal factors.....	13100	48.0
Autosomal loci of dominant lethal factors.....	9800	36.0
All chromosome loci of dominant visible factors.....	175	0.6

This table is impressive even though the estimates on which it is based may be rather crude. The total estimate of 27,000 comparable gene changes furnishes an idea of the complexity of inheritance and lends greater respect, if possible, to the consideration of its functions within the organism. It should not, however, be thought that this large number

represents all of the loci within the chromosomes in any one animal. Our information on gene transmutation has led rather to another view for we find that a single gene is capable of changing into a dominant, a recessive, to one causing viable morphological effect or to one which is lethal. In order not to overstate the number of genes, it would be legitimate to take the largest class as representing the total number of loci within the chromosomes. Such an estimate of the total loci, 14,380, is still impressive. By dividing the total gene chromatin by this number the volume of an average gene is found to be 1×10^{-15} cm.³ The gene would, of necessity, be smaller than this, but if we suppose that it is not, we conclude that each locus would have 120,000 atoms within it (9).

We think of an organism as having reached its present form by virtue of its fitness to survive the intense competition of the ages. This competition in nature has evidently culled carefully, for in looking at the genes found in the wild type fly we see that 92 per cent are so vital to the organism that alteration by X-rays makes them lethal, and yet this may be an overestimate for Demerec has some evidence which indicates that the loss of even a single gene may result in the death of the cell.

The fact that every cell making up an organism may have 14,380 distinctly different entities capable of producing death by mutation gives to the genetic constitution a lethal power which seemingly is unequalled by any other agent.

SUMMARY

The manifold effects of the inheritance of the organism on its reactions to disease and the multiplicity of the separately inherited entities which cause these reactions suggest that a clearer insight into the physical basis of pathology would follow an increasingly better understanding of genetic constitution.

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THE LINGUISTIC ATLAS OF NEW ENGLAND

HANS KURATH

(*Read April 21, 1934*)

I

IN 1929, on the initiative of the Linguistic Society of America and the Modern Language Association, the American Council of Learned Societies charged a committee of linguists to plan a survey of the spoken language of the United States and Canada. In 1930 it was decided that an experimental survey of New England should be undertaken, under the sponsorship of the Council and in collaboration with the Colleges and Universities of New England.

The choice fell on New England for the following reasons:

(1) The speech of New England is, to a considerable extent, the source of the speech of the Middle and Farther West. (2) We are better informed concerning New England speech than concerning the speech of any other section in North America, so that the survey could be planned here more satisfactorily and with greater promise of success. (3) The history of the early settlements and the later history of the population is better known for New England than for any other section, so that communities and types can be more carefully selected and the historical interpretation of the present linguistic situation in New England undertaken on a sounder basis.

By the end of the summer of 1933 the collection of the New England material had been completed, except for the phonographic recording of connected speech, which is in progress now and will be finished by the end of the summer of 1934. Four hundred double faced 12" discs have already been made by Professor Miles L. Hanley, of the University of Wisconsin, at present Lecturer in English at Harvard

University, and 500-600 more will be added to the Atlas collection by the end of the summer. Considerable progress has been made in preparing the New England material for publication. First drafts of manuscripts for 300 of the 800-900 maps are in hand now.

Preliminary field work in the South Atlantic States is being carried on at present by Dr. Guy S. Lowman, Jr., in preparation for a systematic survey of this highly diversified region.

The Southern Survey should in time be followed by a study of the Middle Atlantic States, and finally by a survey of the area to the west of the seaboard states, including the tier of states to the west of the Mississippi River—roughly all of the territory settled by 1850.

The mapping of dialects is not a wholly new undertaking. In Europe, it has been in progress for three quarters of a century, although the greater part of the work has been done in the last three decades. Most of Western Europe has already been mapped. England alone of the Western European countries remains un-mapped.

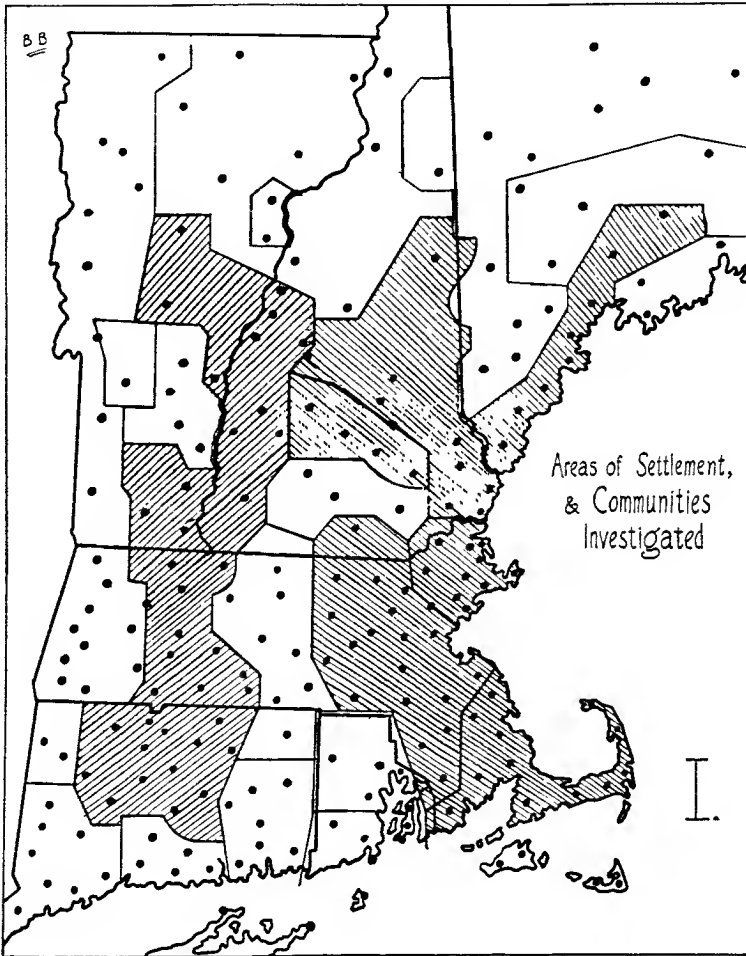
II

Linguistic geography undertakes to ascertain the distribution of linguistic features (dialectal features, if you will)—sounds, words, idioms, inflections, and syntactical peculiarities—and to interpret their distribution historically. Its ultimate purpose is to arrive at an understanding of the development of the speech forms of a country. The speech of a region, a community or an individual cannot be satisfactorily treated without such a general survey.

The first task of the linguistic geographer is to establish the facts of linguistic usage in a given territory.

He may confine himself to the speech of one social class, say the peasantry, or he may investigate the speech of several social groups. The former practice has been followed in all the European countries, but it is obvious that in a country like ours where class distinctions are so vague and shifting

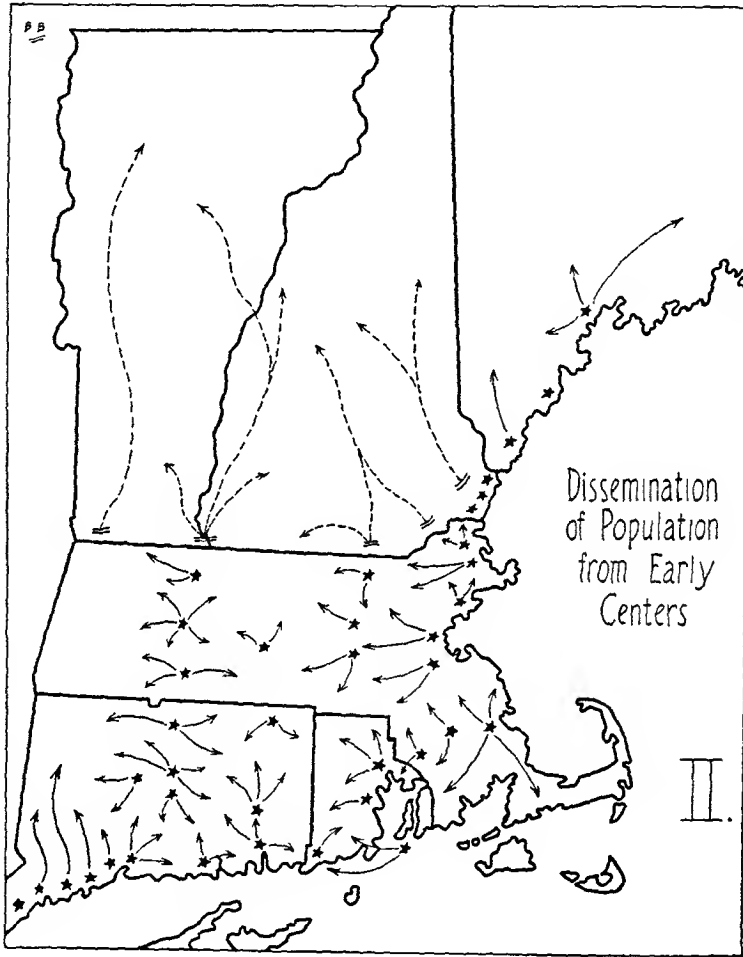
such limitation would be unwise. Hence for the Linguistic Atlas of New England, two representatives have been chosen in each community, one old (70-80) and without much formal education, the other middle-aged (40-60) and better schooled.



Moreover, in about 20 communities a cultured informant was included. The Linguistic Atlas of New England will thus present the geographic distribution of the speech forms of three levels.

In the selection of representative communities the lin-

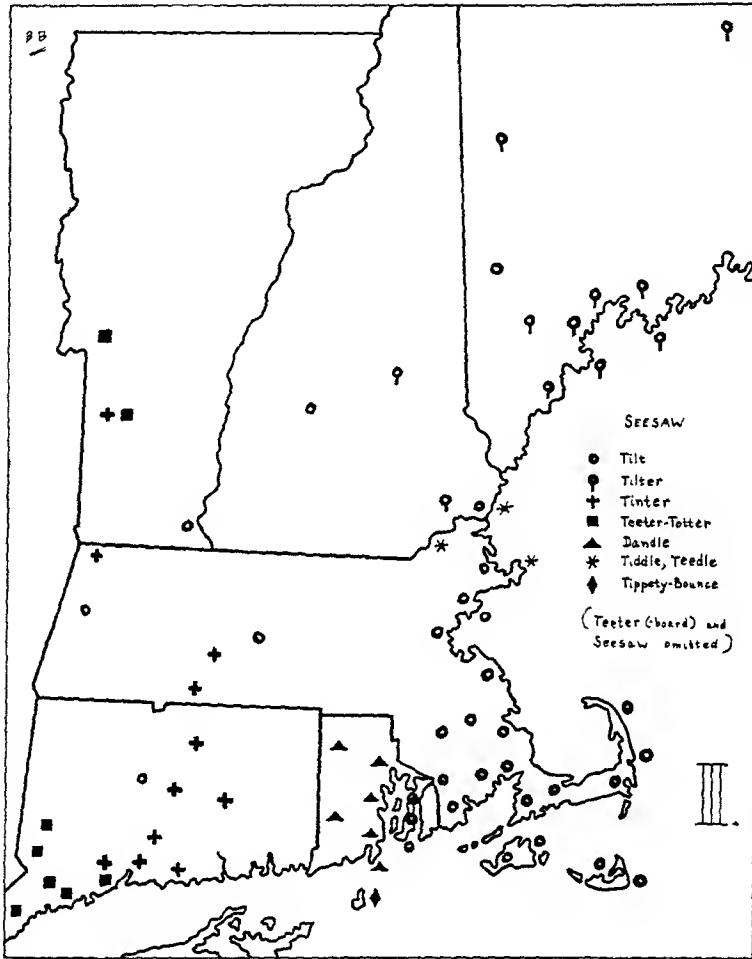
guistic geographer may merely aim at a fairly even distribution or he may also take into consideration the history of the population. The former procedure yields satisfactory results in a close-meshed survey; the latter imposes itself when the



number of vantage points is comparatively small, as in the New England survey. The 225 communities in New England were selected in consultation with a historian, Professor Marcus L. Hansen, of the University of Illinois, who prepared for the use of the Atlas staff an outline of the history of the

settlement of New England by towns and by sections, as well as a bibliography of local history.

Plate I shows the location of the communities in which field records in phonetic notation have been made.

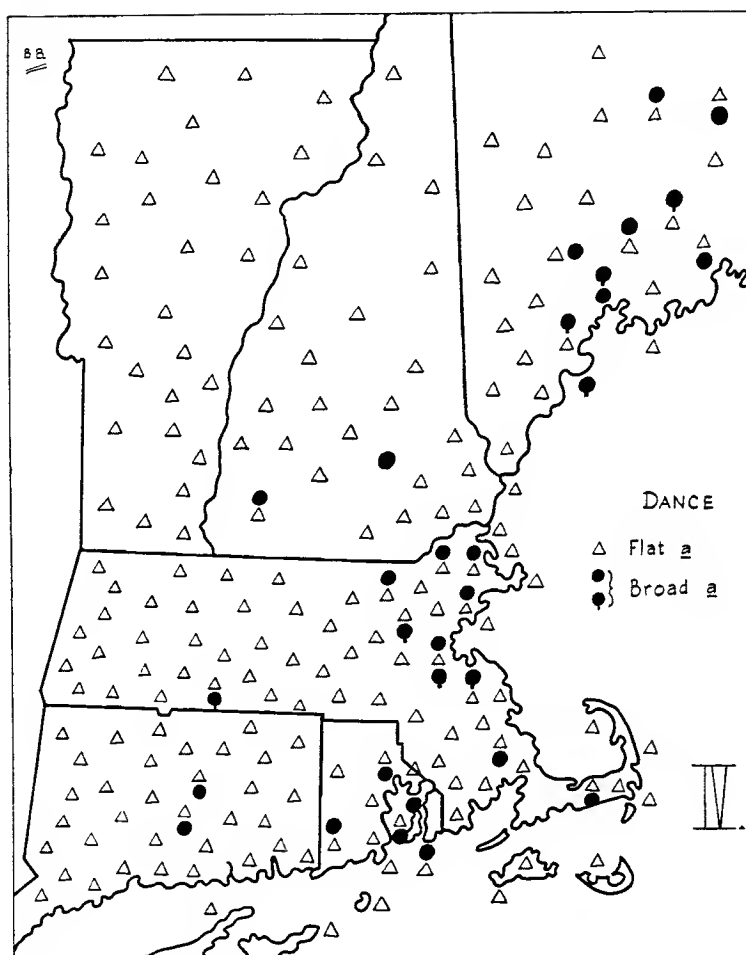


The several areas of settlement with their subdivisions are also roughly indicated on Plate I. The major areas are:

- (1) the coast settlements in the East
- (2) the Connecticut Valley settlements
- (3) the settlements along Long Island Sound

(4) the later western settlements (Berkshire County, Massachusetts and Western Vermont).

Plate II presents, in southern New England, the lines of expansion from the earliest centers and, in northern New England, the general trends of the population movements.¹



These larger areas, as well as some of the subdivisions, are still rather clearly reflected in the present distribution of certain features of dialect (see Plates III-V).

¹ Plates I and II are based upon materials compiled by Professor Marcus L. Hansen for the staff of the Linguistic Atlas.

It is obviously impossible to record the language of any individual exhaustively, if, as in New England, more than 400 informants must be interviewed in the course of two years. One might spend weeks with an informant and yet fall short of completeness. The field worker must know what to look for, if he is to gather a sizeable body of information in a reasonable span of time.

The work sheets must contain sufficient material for a full treatment of the regular features of pronunciation and the grammatical forms, and they should contain a selected vocabulary of the humbler spheres of life in which everyone shares.

In order to focus the attention of the informant upon the subject matter and not upon the expression, the items in the work sheets are topically arranged, so that a more or less connected conversation can be carried on with the informant. The field worker's primary purpose is to establish the informant's natural colloquial usage and not his opinion concerning the propriety of such and such a pronunciation or such and such a term.

Some of the topics with which our work sheets are concerned are the weather, the dwelling, farm buildings, domestic animals (especially the males), calls to animals, crops, food stuffs and their preparation, diseases, personal characteristics, social, religious and political institutions, and various activities of everyday life. Many of the more common irregular verbs, the inflections of verbs, nouns and pronouns, as well as a considerable body of common idiomatic phrases, are thrown into contexts where they are apt to occur; but a good many of these are observed in the course of the interview (which takes from 8 to 20 hours) when the informant is completely off-guard.

The material for each item is presented on a separate map.

The make-up of the maps of the Linguistic Atlas of New England will be similar to that of the *Sprach- und Sachatlas Italiens und der Südschweiz* by K. Jaberg and J. Jud. The forms recorded in the field are entered next to numbers

indicating the location of the communities. Comments of the informants, the field workers and the editors are given with each map. The Linguistic Atlas of New England will have 800-1000 of these large maps.

III

Each feature of speech, whether it be a sound, a word, an inflection or a matter of syntax, has, within limits, its own history, which is reflected in the spread of the variants. Linguistic geography must therefore first ascertain the regional distribution of the variants for each feature separately. It must delimit the central areas (the strongholds) of the several variants and locate areas of transition (mixed areas).

A tentative historical interpretation of the individual features may very well be attempted, but their definitive history cannot be written until after the distribution of the variants of many features has been ascertained, charted, and compared.

If the regional boundaries are fairly definite, as in the folk speech of the European countries, a composite of the maps for a larger number of features will reveal a concentration of linguistic boundaries in some places and a scattering of the boundaries elsewhere. The heavy bundles of lines constitute the chief dialectal boundaries and call for explanation. Is the concentration of boundaries due to original diversity in speech (as in the case of the transplantation of population) or was it produced by a barrier to free communication, such as a political boundary (town, county, or state line), an economic boundary (limits of trade areas), a cultural boundary (religious affiliation) or some physical barrier (a mountain range, a body of water, lack of roads)?

For lack of information, no study of this type has as yet been undertaken in the English speaking countries.

In this country and in English speaking Canada there are few, if any, clear pronunciation boundaries (isophones) and word boundaries (isoglosses), but there are distinctive central areas (*e.g.* Plymouth Colony, Massachusetts Bay Colony,

Fairfield and Western Litchfield County in Connecticut), transition zones, and highly mixed areas (*e.g.* Worcester County, Massachusetts and the upper Connecticut Valley). A composite of the distribution charts of certain features of speech will, I am confident, make it possible to delimit such areas.

That speech is in constant flux is a matter of common knowledge, but the precise nature of the changes, and the forces behind them, are often hard to determine and only imperfectly understood.

No one really doubts that some changes are indigenous, while others are due to importation; but the explanations offered for a vast number of individual cases of change are, in the absence of conclusive proof, largely dependent upon certain preconceptions (or working hypotheses).

The older school of linguists (especially the Junggrammatiker) were inclined to believe in "internal development" in the absence of proof to the contrary. The linguistic geographers on the other hand (and some earlier scholars like J. Schmidt and H. Schuchardt) would rather assume "spreading," *i.e.* importation of speech forms from without, unless the contrary can be proven or made plausible.

The work done in the field of linguistic geography, especially in France and in Germany, has demonstrated the surpassing importance of the "spreading" of speech forms.

Spreading may take place (1) along an unbroken front (*i.e.* by expansion) or (2) by leaps, as it were (*i.e.* by radiation). In the latter case, the spreading speech form is first adopted among the upper classes in the larger centers of population, whence it spreads by "infiltration" to the lower levels in the city and to the country-side. In the end the spreading circle reaches the area whence the innovation came, so that ultimately the result is the same as in the case of unbroken expansion.

Spreading by leaps is characteristic of the speech forms of cultural centers.

The spreading of speech forms from the literary language

into the local dialects is akin to "spreading by leaps." But since the literary language is often not bound up with any locality but rather with a social group scattered over many communities, the spreading takes the form of "infiltration."

Infiltration of speech forms from the spoken or the written standard language into popular speech is a common phenomenon everywhere. The "standard forms" pass from the upper levels of speech to the lower levels. They become established faster in urban (thickly populated) areas than in rural (thinly populated) areas.

Spreading of all types, as I have used the term, is independent of population movements or migrations. It is the result of communication.

Speech forms are, of course, also carried from one place to another through the migration of peoples such as that of the English colonists who came to this continent. In New England the original settlements and the expansion of the population during the Colonial Period and later is clearly reflected in the distribution of dialectal features of speech to this day.

The original differences in dialect among the early settlers of New England, who came from various parts of England in the 17th century and later also from Northern Ireland, probably account for the majority of the regional variations in present-day New England.

Even now when but a few of the nearly thousand items of the Linguistic Atlas of New England have been charted and analyzed, the origin and the spreading of the dialectal variations in New England can in many cases be inferred from their present distribution.

Variations owing to diversity in the speech of the early colonists tend to coincide with the original settlements and the areas populated from them.

The words for the seesaw have a distribution of this type (see Plate III). *Tilt* is concentrated in the Plymouth Colony and the coast towns of the Massachusetts Bay Colony; *dandle* is confined to Rhode Island west of Narragansett Bay;

tiddle and *teedle* occur only in Essex County, Massachusetts, and in an adjoining town in New Hampshire; *tilter* appears primarily along Casco Bay; *teenter* and *tinter* are used in the New Haven Colony and in the southern part of the Connecticut Valley; and *teeter-totter* turns up only in Fairfield County, Connecticut, and in southwestern Vermont. Besides these local terms *tecter* and *seesaw* are widely used, the latter being clearly an "infiltration" from the literary language.²

The distribution of the words for the earthworm also reflects some phases of the history of the settlement of New England: *caseworm* occurs only in Providence County, Rhode Island, and in the Rhode Island settlements in northwestern Berkshire County; *angledog* is concentrated in the Windsor Colony in Connecticut; and *mudworm* is fairly well confined to the Merrimack Valley.³

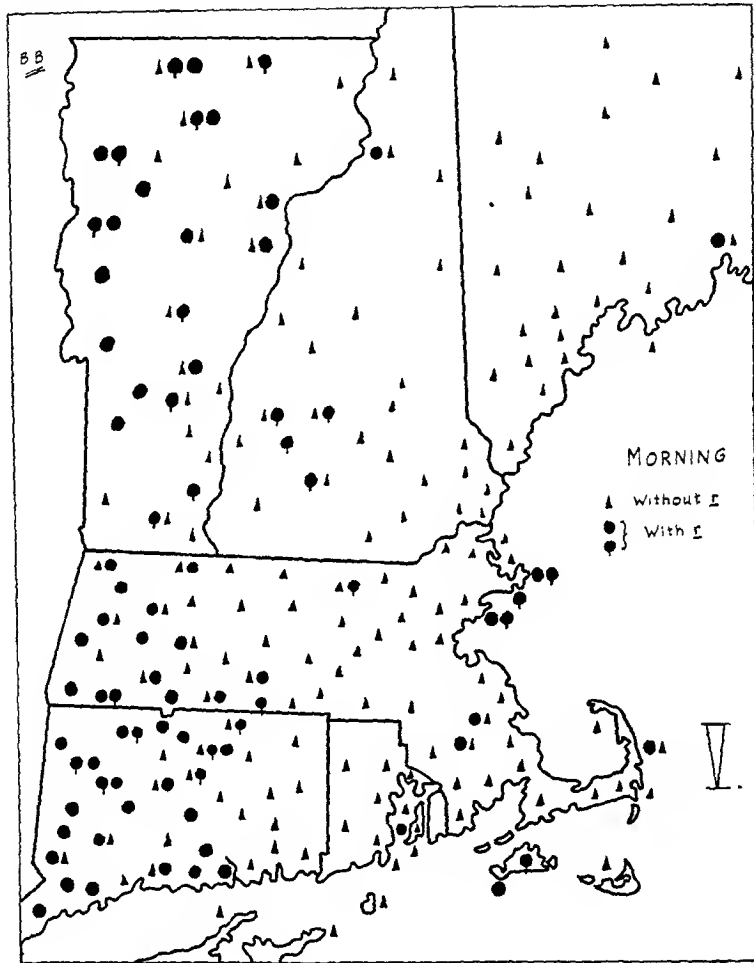
The pronunciation of the vowel in *dance* furnishes another striking example of the effect of the original settlement of New England upon the present distribution of speech forms (see Plate IV). The type of *dahnce* is concentrated in the coastal section of the Bay Colony, in the adjoining Essex County and along Narragansett Bay. From Massachusetts it was transplanted to Casco Bay and to eastern Maine. It is a striking fact that in the New Hampshire coast towns and York County, Maine, *dahnce* is not current, while in the Plymouth Colony it appears only as an upper-class pronunciation, which was doubtless introduced by "radiation" from the Boston area. Radiation accounts also for the occurrence of *dahnce* in the cities of other sections of New England, namely Concord and Keene, New Hampshire, Springfield, Massachusetts, and Hartford and Middletown, Connecticut.

The present regional distribution of the pronunciations of *morning* with and without *r* (see Plate V) must ultimately depend upon the importation of different dialectal types from England and northern Ireland. Generally speaking, the

² See my discussion of this item in *American Speech*, April, 1933.

³ For a full discussion of this item see Rachel S. Harris, *American Speech*, December, 1933.

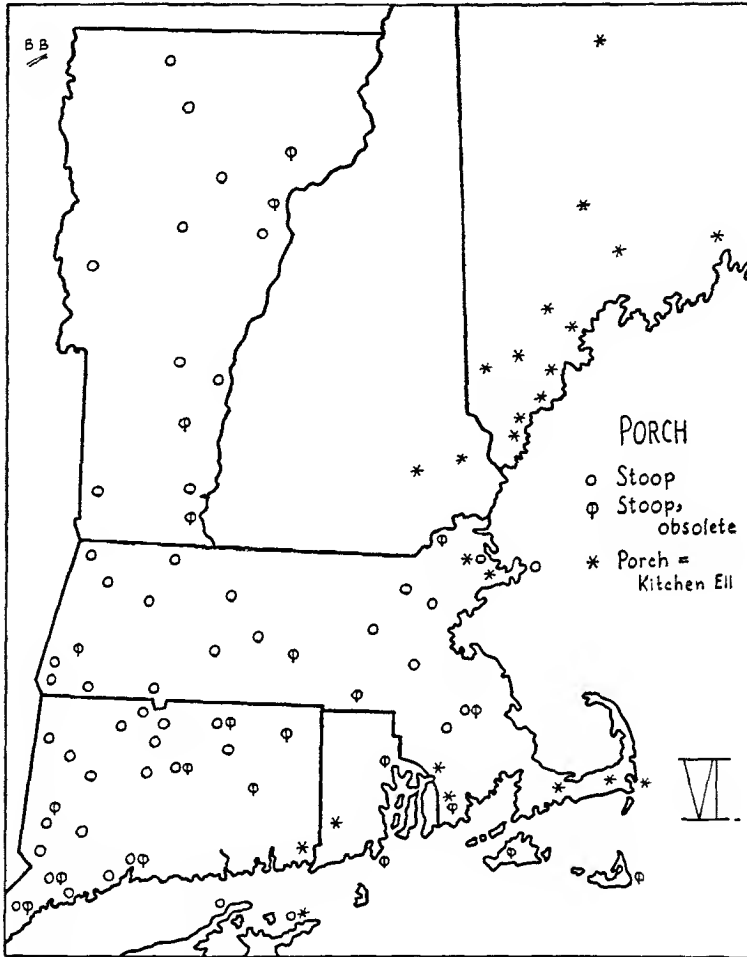
eastern two thirds of New England have the *r*-sound only before vowels, while the western third has the *r* in all positions. In early colonial days both types probably existed side by side in most of New England, but in different proportions.



By a process of spreading, one type gradually became established in the East, the other in the West. The East now agrees with southeastern England, whence the majority of the early settlers came.⁴

⁴ See C. O. Paullin and J. K. Wright, *Atlas of the Historical Geography of the United States*, Plate 70 D.

R-islands still exist in eastern New England, notably Marblehead and Cape Ann, Martha's Vineyard, and the Scotch-Irish settlements in New Hampshire and along the



Maine coast; but the rising tide of the r-less type will in all probability submerge them before long.

Dialectal variations which are due to "expansion" (as defined above) center about particular regions that do not necessarily coincide with areas of settlement.

The Dutch word *stoop* "expanded" into New England

from the Hudson Valley, probably as the name of a particular type of porch (see Plate VI). It is important to keep in mind that the spreading of the word eastward runs counter to the westward trend of the New England population. The original home of *stoop* can be readily inferred from its present distribution; it becomes more and more scarce as one goes east. Its complete absence in Maine makes it safe to conclude that *stoop* did not reach eastern Massachusetts until 1775 and perhaps not until much later.

Outlying and secluded districts are the last to be reached by the spreading waves of innovation. The r-islands in eastern New England illustrate this fact. An interesting case of survival is *porch* with the meaning "kitchen ell" (see Plate VI). The occurrence of *porch* in this sense in scattered communities all along the coast from eastern Connecticut to eastern Maine permits one to infer that it was formerly in use throughout eastern New England.

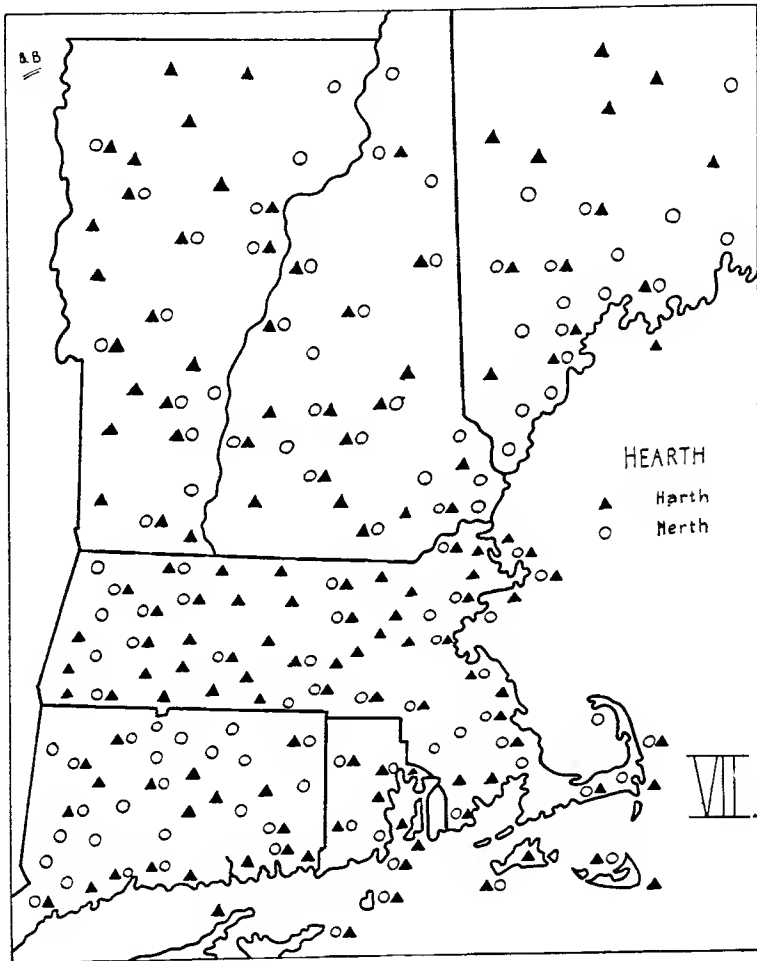
Innovations that spread by "radiation" (as defined above) can be identified by their scattered occurrence in cities outside the home-area of the spreading word or sound and in the neighborhood of centers of population.

The pronunciation *dahnce* in the cities of the Connecticut Valley is doubtless due to radiation from the Boston area (see Plate IV).

The influence of Boston speech appears clearly in southern Essex County and in the northern part of the Plymouth Colony both in matters of pronunciation and in the use of words.

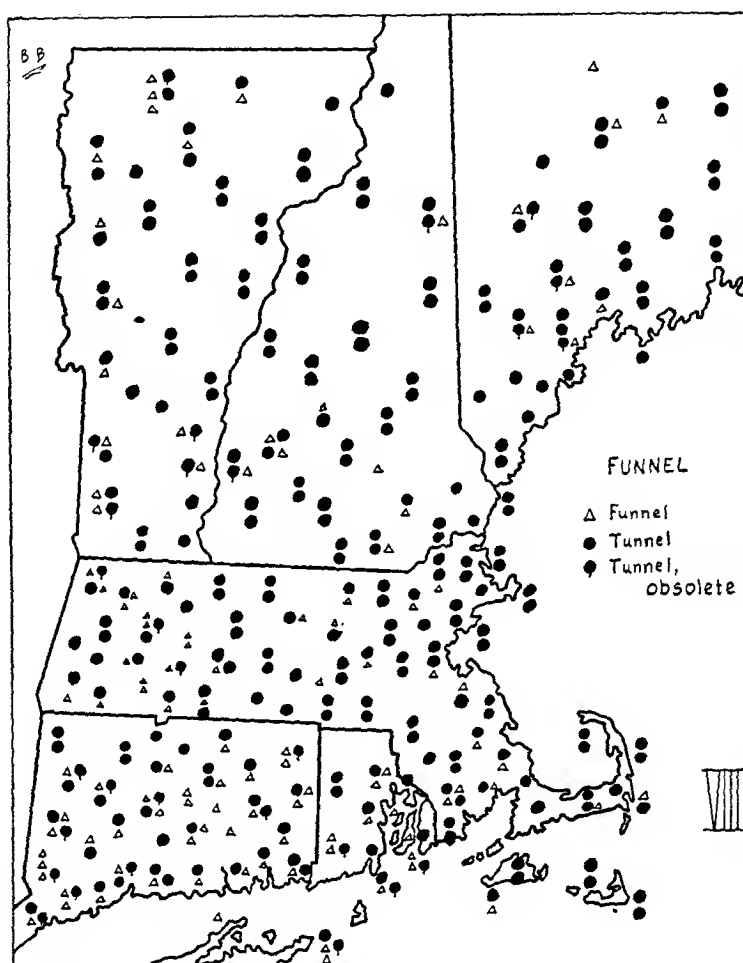
Infiltrations from the standard language betray themselves in divided or unsettled usage in a large number of communities in all parts of New England. Such innovations are largely disseminated by the schools and, of course, by printed matter. Innovations of this type have no one geographic center of dispersion. They make their appearance in a large number of communities at one and the same time, first in the more thickly populated areas, last in rural sections. Plate VII illustrates this type of spreading. The old pronunciation of

hearth as *herth* is being replaced by the new pronunciation *harth*. It is interesting to observe that Connecticut and western Massachusetts, which have more intimate contact



with metropolitan New York, are less conservative than the Boston area. A similar situation is revealed in the present distribution of *funnel* and *tunnel* (in the sense of "funnel"), the former of which is spreading by infiltration from the literary language (see Plate VIII).

In a recent study of the New England words for "poached eggs,"⁵ Herbert Penzl points out that the literary term *poached eggs* is beginning to replace the local *dropped eggs* in



the cities of eastern New England, whereas in western Connecticut *poached eggs* is fully established (page 93). Is it native to Connecticut or was it imported by radiation or expansion from Metropolitan New York? Mr. Penzl assumes

⁵ *American Speech*, April, 1934.

that it is native. He may be right. However, a final answer can hardly be given until other cases have been studied and probably not until a survey of New York State is made.

The solution of many problems in New England speech will be made possible by the publication of the Linguistic Atlas of New England; but others will remain in suspense until other sections of the country are surveyed.

BROWN UNIVERSITY

WHAT IS *ÆNOTHERA HOOKERI* TORREY & GRAY?¹

BRADLEY MOORE DAVIS

(Read April 19, 1934)

THERE are in the genetical and cytological literature on *Ænothera* extensive references to a plant that Professor deVries (1913) early established in his experimental garden under the name *Ænothera Hookeri* Torrey & Gray. It came from seed collected near Berkeley, California, and the plant with related forms has a wide range on the Pacific Coast (Gates 1915).

Ænothera Hookeri was described by Torrey and Gray in the "Flora of North America," 1838-40. Watson (1873) reduced *Hookeri* T. & G. to the synonymy of *Ænothera biennis* var. *hirsutissima* Gray (1848), a disposition which Brewer and Watson (1880) followed in the "Botany" of the "California Geological Survey." Small (1896) writes of the species as *Onagra Hookeri* (T. & G.) Small, noting that it is far removed from *Ænothera biennis*. Rydberg (1922) recognizes *Ænothera Hookeri* T. & G. and describes a species *Ænothera hirsutissima* (A. Gray) Rydb. distinguished from *Hookeri* by very short sepal tips (2-2.5 mm.), a heavier pubescence, shorter leaves, and longer capsules. Jepson (1925) under *Ænothera Hookeri* T. & G. writes a description broad enough to include *Ænothera franciscana* Bartlett (1914) and a number of allied forms, among them the *Hookeri* of deVries. It is hoped that in this assemblage the true *Hookeri* Torrey & Gray will be found and from the assemblage segregated in the experimental garden. Professor deVries in assigning his plant to *Ænothera Hookeri* T. & G. followed the practice of the period in agreement with Jepson's treatment of the species.

DeVries's plant is now known to many workers with *Ænotherae* and is one of the most interesting types in culti-

¹ Genetical Studies on *Ænothera*, XVII. Papers from the Department of Botany, University of Michigan, No. 485.

vation because it is homozygous, having free pairing chromosomes, almost perfect pollen and seed fertility, and because it breeds true. For these reasons the plant has been treated as a key or base type by which through cross breeding and cytological studies much has been accomplished in the way of an understanding of *Ænothera* genetical complexes and chromosomal configurations. It is the purpose of this paper to show that the plant which we will discuss as *Hookeri* of deVries is not *Ænothera Hookeri* Torrey & Gray.

I am greatly indebted to Dr. Robinson for much assistance in my examination of material and historical references at the Gray Herbarium.

Ænothera Hookeri Torrey & Gray

There is a specimen in the Gray Herbarium which Dr. Gray undoubtedly knew and probably handled at the time when the description of this species was written, and which consequently may be regarded as representing the type. A photograph of this specimen is reproduced (Plate I) and its characters will be discussed in comparison with the plant of deVries's, but, before this material is presented, certain matters of history should be considered.

The label on the sheet bearing the specimen holds the following—in print “Herb. A. Gray,” in the handwriting of Dr. Gray “*Æ. odorata*. Hook. Beech.” and below “*Æ. Hookeri*,” in print “Torr. & Gray, Fl. N. Amer.,” and at the bottom of the label, again in the handwriting of Gray, “California, Douglas (B.).”

The original description of *Ænothera Hookeri* from Torrey and Gray “Flora of North America,” vol. 1, p. 493, 1838-40 reads as follows:

“5. *Æ. Hookeri*: canescently pubescent and somewhat villous; stem erect, angled; leaves lanceolate, sessile, rather acute, obscurely denticulate, not undulate; flowers (large) sessile, in a leafy spike; calyx villous; the tube twice the length of the ovary, rather shorter than the slightly-acuminate segments; petals obcordate, about the length of the style; stigmas linear, somewhat thickened; capsules short.—*Æ. odorata* ? Hook. & Arn. bot. Beechey, suppl. p. 343, scarcely of Jacq.”

"California, *Douglas*!—Stem stout and tall, strict, strongly angled: pubescence soft and minute, with long and coarse hairs intermixed. Petals apparently yellow, turning to rose color. Ripe fruit unknown.—This plant differs from *Æ. odorata* (which is said to be a native of Patagonia) in its plane leaves, which are not attenuated to a sharp point, its perfectly sessile ovaries, etc., and is besides more hairy. In the collection of Dr. James, made near the sources of the Platte or Canadian, we have a fragment apparently of the same species."

The comment at the end of the first paragraph "*Æ. odorata*?" together with what follows refers to Hooker and Arnott "The Botany of Captain Beechey's Voyage," "California—Supplement," p. 343, with "12. *Æ. (Euœnothera) odorata*. Jacq.? The leaves are rather more hairy than usual; but we perceive no essential difference." This is in Part 8, pp. 337–384 which was published in 1840 or earlier. The plant of course is not *Ænothera odorata* Jacq., which is now often placed in the genus *Raimannia* and is native to South America.

The statement "California, Douglas!" on the label and at the beginning of the second paragraph is important. It refers to the introduction to the "California—Supplement" page 316 of "The Botany of Captain Beechey's Voyage" which reads "Where not otherwise mentioned, it is to be understood that the following species are from the collection of Mr. Douglas. They were presented by the Horticultural Society of London, in whose service Mr. Douglas was at the time that he gathered them."

It is clear that we are dealing with a specimen collected by David Douglas² who was sent to America by the Horticultural Society of London and was in British Columbia in 1825–27, in California 1830–32, and on the Fraser River 1832–33. He is reported to have collected 800 species in California and to have introduced 217 new species into England. The statement "California" on the label must be taken broadly to include an area larger than the present state of that name.

² The historical matter in this paragraph is from Britten and Boulger "A Biographical Index of British and Irish Botanists," 1893.

There are other specimens of *Ænothera* from the American collections of Douglas and some of them might give additional evidence on the characters of *Ænothera Hookeri* T. & G. Two of these are in the Lindley Herbarium of Cambridge University and have been discussed by Davis (1926) in connection with the early introduction into England (previous to 1830) of large-flowered, narrow-leaved *Ænotherae* with pubescence similar in character to that of *Lamarckiana*. Gates (1915) gives references to others. It is hoped that all Douglas specimens of *Ænothera* from the Pacific Coast will be re-examined and compared with the following description and photograph of the type in the Gray Herbarium. Perhaps some of them may prove to be so similar as to be possibly duplicates of the type.

A COMPARISON OF THE DOUGLAS SPECIMEN OF *Ænothera Hookeri* T. & G. WITH THE *HOOKERI* OF DE VRIES

We are of course limited in this comparison to the characters shown in the specimen from the Douglas collections on which must have been based the description by Torrey and Gray in the "Flora of North America," but there is enough material to show that the two plants are different species.

<i>Ænothera Hookeri</i> T. & G.	<i>Hookeri</i> of de Vries
Leaves. Those just below inflorescence narrowly lanceolate, the greatest breadth about 10 mm. Heavily pilose and puberulent appearing canescent.	Leaves. Those just below inflorescence lanceolate, the greatest breadth about 18 mm. Sparsely pilose and puberulent.
Stem. Heavily pilose and puberulent, approaching pilose. Red papillæ.	Stem. Much less heavily pilose and puberulent. Red papillæ.
Inflorescence. Compact. Bracts $\frac{1}{3}$ - $\frac{1}{2}$ length of mature buds.	Inflorescence. More open. Bracts $\frac{1}{3}$ - $\frac{1}{2}$ length of mature buds.
Buds. The largest 6.7 cm. long (probably not mature as indicated by the small ovary). Cone 3.2 cm. long, 8 mm. wide at base.	Buds. 7.5-8 cm. long. Cone 3-3.4 cm. long, 8 mm. wide at base.
Sepals. Apparently light red. Heavily pilose and puberulent appearing canescent.	Sepals. Dark red. Less heavily pilose and puberulent.
Sepal tips. 4 mm. long on the detached bud. Thick, appressed, blunt, green. Heavily pilose.	Sepal tips. 4-5 mm. long. Thick, appressed, blunt, green. Less heavily pilose.
Petals. 3 cm. long.	Petals. 3-3.5 cm. long.
Stigma lobes. 4-5 mm. long, 6 mm. above tips of anthers.	Stigma lobes. 4-5 mm. long, 6-8 mm. above tips of anthers.

The species *Ænothera Hookeri* Torrey & Gray differs from deVries's plant in having much narrower leaves and a much heavier pubescence which is, however, similar in being pilose and puberulent. The puberulence on the leaves and sepals is canescent while the long hairs give to the stem almost a villose appearance. Points of resemblance are to be found in the similar buds, and the large flowers with stigma lobes well above the tips of the anthers. Most important is their agreement in having sepal tips thick, appressed, blunt, and green. This type of sepal tip is characteristic of a group of forms found in California of which deVries's *Hookeri* and *Ænothera franciscana* Bartlett are well known to *Ænothera* geneticists. This agreement on sepal tips, an important character in *Ænothera* taxonomy, holds these species in an assemblage that contrasts with certain other Pacific Coast forms that have long, pointed sepal tips inclined to separate from one another.

Judging from the Douglas specimen it seems probable that *Ænothera Hookeri* T. & G. will be found to be a plant with a foliage of unusually narrow leaves both in the rosette stage and at maturity. The pubescence will be exceptionally heavy, on some parts of the plant canescent, and on the stem it may approach villose. In many respects the species will resemble deVries's *Hookeri* and *Ænothera franciscana* Bartlett, but other peculiarities will probably appear.

The taxonomic situation within the assemblage of forms included in Jepson's (1925) treatment of *Ænothera Hookeri* is strangely confused. Only one type has been adequately described in taxonomy and this is *Ænothera franciscana* Bartlett (1914) which in my cultures has been carried through 16 generations and has been the subject of extensive genetical studies that have established its constancy as a homozygous type. This species must be the starting point in any taxonomic treatment of the assemblage. The *Hookeri* of deVries is a closely related form but worthy of separation from *franciscana* as a homozygous biotype which in critical taxonomy must eventually be recognized. The true *Ænothera*

Hookeri Torrey and Gray will probably be found and when properly tested in the experimental garden will establish this species in systematic botany.

MARKED DIFFERENCES BETWEEN *Ænothera franciscana*
BARTLETT AND THE *HOOKERI* OF deVRIES

Below are given the most important distinctions between *Ænothera franciscana* and the *Hookeri* of deVries as shown in garden cultures. They are based on observations covering seven generations of a line of deVries's *Hookeri* from seed kindly given to me by Professor Renner in 1924 in comparison with my line of *franciscana* derived from Bartlett's cultures of 1912.

<i>Ænothera franciscana</i> Bartlett	<i>Hookeri</i> of deVries
Mature rosette. Leaves broadly elliptical, somewhat crinkled; 20-25 cm. long, 4-4.5 cm. broad; petiole 3-5 cm. long.	Mature rosette. Leaves narrowly elliptical, plane; 25-30 cm. long, 2.5-3 cm. broad; petiole 6-7 cm. long.
Mature plant. Central shoot about 9 dm. high, more densely branched. Side shoots from rosette 6-7 dm. long. Young stems green, becoming light red with age.	Mature plant. Central shoot about 6 dm. high, sparsely branched. Side shoots from rosette 4.5-5.5 dm. long. Young stems light red, becoming dark red with age.
Foliage. Leaves on upper parts of plant lanceolate, broad at base.	Foliage. Leaves on upper parts of plant elliptical, less broad.
Mature buds. 8-9 cm. long. Cones 31-35 mm. long, 10 mm. thick. Sepals light red. Hypanthium with areas of light red.	Mature buds. 7.5-8 cm. long. Cones 30-33 mm. long, 8 mm. thick. Sepals dark red. Hypanthium with areas of dark red.

It is hoped that this discussion will stimulate search for *Ænothera Hookeri* Torrey and Gray. Seeds of possible representatives should be collected and sent to workers concerned with *Ænothera* genetics and taxonomy that proper comparative studies may be made.

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PLATE I



Type of *Euthamia heterophylla* Torr. and Gray. This specimen in the Gray Herbarium was collected by David Douglas on the Pacific Coast of North America and on it Torrey and Gray must have based their description of *H. heterophylla* in the "Flora of North America," 1840.

NORMAL VARIATION

ALEŠ HRDLIČKA

(Read April 21, 1934)

THE phenomenon of normal variation means that, outside of everything pathological or incidental, all parts and all functions, of every known form of organism, vary, and that within definite limits for each part, each species and under given basic conditions. Normal variation appears therefore to be a universal and inherent condition of organic existence.

On close scrutiny the normal variation of everything organic is found to be one of the most important properties of living matter. It is plainly the essential factor in natural selection, and is apparently the essential condition of organic evolution as a whole. Its extent and characteristics in man are therefore of the utmost interest to physical anthropology; the latter, in fact, is fundamentally the study of human variation.

As variation is seen to extend to all organisms, and that both in structure and function, it would seem that variation must necessarily also extend to the microorganisms causing disease, and hence to the affections of structure as well as function, or to pathology. The study of variation in this field is still, however, in infancy.

Rational understanding of normal variability is as yet impossible; but followed logically the phenomenon would ultimately and inevitably seem to lead to the variability of the organic molecules.

There is as yet no adequate definition of normal variation. An appeal I made in 1925 to a dozen of the most prominent contemporaneous workers in biology, genetics and anthropology, including such men as Conklin, Osborn and Bateson,

resulted in a series of interesting replies but no clear statement or explanation.¹ The whole subject is in fact still far from being well understood in any line of the natural and especially the medical sciences, and is often ignored even by otherwise prominent workers.

Superficially, normal variation may be defined as that range of variability of any organism or a part of an organism, which can not be attributed to abnormal factors; and by abnormal factors must naturally be understood all the mechanical, nutritional and pathological agencies that act incidentally and disturbingly on any individual. In other words, if we exclude from a group studied all the results of mechanical appliances, malnutrition, peculiar habits, accidents and diseases, there still remains in every group and for every part of the body a range of variation which, if the series be large enough, gives a regular curve of distribution, is substantially inherent, is not readily affected by any ordinary means, and can therefore be called only normal.

The phenomenon of normal variation is so basic and so absolutely universal—so far at least as the organic world is concerned—that it must be regarded as one of the very basic phenomena and laws of living matter. There are some indications that in some form it may extend even to what is termed inorganic nature, but this is beyond our present concern. Moreover, it is apparently not a simple law, not one of a simple mathematical nature.

An analysis of normal variation shows it to be modifiable to a degree by a series of potential normal factors. The principal of these factors are age, sex, race, group, and function. They often act concurrently and even conjointly. All these factors together, but more especially function, may increase, but apparently never decrease variability. Eliminating everything that may affect the process ontogenetically, a certain range of what may be termed fixed variation still remains, and can be referred only to long inherent factors.

What these long inherent potentialities are, is still very

¹ For the replies see *Am. J. Phys. Anthropol.*, 1925, VIII, 437-442.

uncertain. As the search reaches further into the subject the realities become dimmer and less comprehensible. Nevertheless another step is possible. It may be conceived with some legitimacy that some proportion of these inherent potentialities of variation are merely ancient habitual functional responses that have become hereditary. But after a liberal allowance for this proportion there still remains in probably all cases an important residuum which can not as yet be satisfactorily explained. Even if hypothetically all the variation of man and other living forms were attributed to ultimate functional and genetic sources, there would still, from analogies, exist the practical certainty that the very first living protocell, if it could have been examined in numbers, would have shown its range of normal variation.

Taking into critical consideration everything that is known of the subject of organic variation or that can logically be reached, there appears no escape from regarding the phenomenon as universal in the organic kingdom and as one of the fundamental properties of every organism and every part of every organism. This necessarily leads to the assumption—for which there is already some substantiation—that even every separate kind of a cell varies normally. From which another logical step would lead to the theory that every separate organic constituent, hence every chromosome and gene and organized granule, varies; and still further to the possibility that normal organic variation in its finality depends upon the variation of the organic molecule. Nor could this conception, if ever proved true, stop with organic matter. The gist of this is a flight into the shadowy realms, the elucidation of which belongs to the future.

Let us return to the humbler task which regards variation especially within the human family. Here one of the first problems that demands attention is the mechanism of variation. This problem, though badly neglected, seems capable of fair presentation.

Variability—whether that of the whole body, as in the case of stature or weight; or that of an organ, such as the eye

or the brain; or that of a feature, such as the nasal aperture, the crown of a tooth, a definite process of or a hollow on a bone; or that of a function, such as the heart beat, secretion, sleep, etc.—variability shows two constituent entities, one the part in which the variation is realized, and the active something which brings about and determines the variation.

The part itself, whatever it may be, is composed of the material constituents, and of the living matter. The former is inert, passive; the latter active, but acting generally under strict control of and regulation by a highly specialized entity, the nervous system. The normal variation of a part, or of the whole body, must necessarily be the resultant of the variation in these three entities—the basic substances, the living constituents and the controlling agency. That proportion of normal variation which is of organized and regularly recurring nature can only be attributed, it would seem, to the nervous centers.

It appears safe, therefore, to conclude that the motives and the directive of normal variation in all parts proceed primarily from the endowments and activities of the nervous system of the human as well as any other organism. Normal variation in this light may hence be designated as essentially the result of the properties and endowments of the nervous system of a given class of individuals.

Perhaps it is possible to go a small step further in this direction. It is not yet definitely known as to just what parts of the general nervous system control growth and function, in general or in particular. Judging from what has been learned on the subject through teratology and other pathological processes, such controls are probably connected with the so-called trophic centers, are widely disseminated and complex, and have their seats essentially in the older parts of the nervous organs.

Beyond referring the sources of normal variation to the trophic centers in the nerve ganglia in the spinal cord and in the brain, and connecting them with probable variations in the active constituents of the cells of these centers, valid analysis for the present is impossible.

THE BEHAVIOR OF VARIATION

We may now approach the actual manifestations of normal variation in man and note their behavior.

One of the first important lessons learned in this field is, that normal variation of the body as a whole, or any of its parts that have some individuality, may be both gauged and measured. This is done by its curve. Every determination on the body, if sufficiently accurate and detailed and extending to an adequate series of subjects or specimens, gives when arranged by grades a characteristic aggregation which may be expressed graphically by a curve. This curve gives simultaneously the range of the variation of the feature dealt with, the median or point of maximum frequency, and the nature of the distribution of the variants. It is an invaluable aid both in study and comparison—provided always that it extends to adequate numbers and that it has been constructed so as to give the best results, points which are still far from being duly and generally understood.

With the help of these curves it is now possible both to determine the needed size of the series, and to eliminate the pronounced abnormals.

As to the size of the series, it has been learned that for a proper presentation of variability there are required at least 200 normal subjects or specimens of one sex and one age group. Series of 500 to 1,000 are especially prized. Data on small numbers, even down to a half dozen specimens, are not without value, but are not fit for the study of variation.

Pronounced abnormals, even with large series, will generally be found to stand outside of and disconnected with the curve. Such occurrences call for elimination.

The minimum to maximum in a good series constitutes the range of normal variation and this for purposes of comparison may be expressed in different ways mathematically. The simplest yet serviceable method is to present it in its relation to the average of the series, which is done by dividing the range by the average ($R \times 100 / \text{Average}$). The result may be termed a simple coefficient of variability, is readily under-

stood, and is very useful for rough comparisons. The standard deviation with the probable error offers a more complex method but one without evident correspondingly greater benefits.

Testing the data on the different parts of the human body in such manner, the first realization is that every part, every dimension, every feature, has its own magnitude or range of normal variation: that this magnitude or range remains the same in repeated adequate series of the same group; that it shows often close similarities in different human groups, even races; and that, while various parts or determinations evidently correlate in their variability, others differ widely.

The range-average coefficient of variation in the measurements of the Old Americans¹ differed from 12.85 for the mean head diameter to 72. — for the ear index and 94.3 for the weight of the body, and was still higher for certain of the bodily functions. It will be useful, I believe, to reproduce here the whole table.

OLD AMERICANS: ABSTRACT OF VARIABILITY

<i>Males</i>	<i>Least Variable</i>	<i>Females</i>
Height sitting-stature index.	13.7	Mean head diameter. 12.85
Mean head diameter	15.2	Height sitting-stature index. . . . 14.6
Length of lower limbs	15.4	Head length 15.0
Trunk length	16.9	Cephalic index. 17.3
Head length.	17.2	Facial module, morphologic. 17.5
Facial module morphologic	17.8	Head breadth. 17.6
Facial breadth (diameter bizygomatic maximum)	18.0	Facial module, physiognomic. 17.75
Head height.	18.7	Length of lower limbs. 17.8
Facial module, physiognomic.	19.3	Head height. 18.0
Cephalic module-stature index	20.0	Trunk length. 18.9
		<i>Stature</i> 19.65
<i>Moderately Variable</i>		
Head breadth.	20.0	Facial breadth, (diameter bizygomatic maximum). 20.0
Mean height index of head.	20.5	Cephalic module-stature index . . . 20.05
<i>Stature</i>	20.7	Hand index. 20.9
Facial index, physiognomic	24.1	Head, mean height index 21.2
Hand index.	24.1	Hand breadth. 21.6
Arm spread.	24.5	Facial height, lower (M-N). 21.6

¹ From *The Old Americans*, Svo, Baltimore (Williams & Wilkins Co.), 1925, pp. 393-394.

Diameter frontal minimum	24.6	Diameter frontal minimum	21.7
Facial height, total (M-C)	24.9	Facial height, total (M-C)	22.8
Facial height, lower (M-N)	25.2	Arm spread	23.3
Cephalic index	25.8	Foot length	24.9
Hand length	26.4	Gonio-frontal index	25.0
Foot length	26.8	Facial index, physiognomic	25.5
Temperature	28.4	Foot index	25.7
Foot index	28.6	Temperature	26.3
Facial index, morphologic	29.3	Hand length	26.5
		Diameter bigonial	28.5
		Ear breadth	28.8
		Facial index, morphologic	29.4
		Ear index	29.8

Variable in a Higher Degree

Foot breadth	30.6	Ear length	30.5
Diameter bigonial	33.9	Nose breadth	30.8
Gonio-frontal index	34.3	Foot breadth	31.1
Hand breadth	35.9	Leg, girth	34.4
Nose breadth	36.0	Nasal index	36.8
Nose length	37.4	Nasal length	38.4
Mouth width	39.1	Forehead, height	40.3
Ear breadth	40.9	Mouth, width	40.4
Leg, girth	42.6	Chest index	44.7
Chest index	44.9	Chest module	46.8
Pulse	45.3	Chest breadth	47.4
Chest breadth	46.4		
Chest module	48.2		
Forehead, height	48.6		

Most Variable

Nasal index	55.7	Chest, depth	57.4
Ear length	58.3	Pulse	60.9
Chest, depth	64.5	Respiration	65.9
Ear index	72.0	Weight*	94.3
Weight*	72.2	Strength: pressure {right hand	116.0
Respiration	75.8	left hand	128.9
Strength:		Traction	181.0
Pressure {right hand	88.5		
left hand	90.0		
Traction	143.5		

There are in normal human variation notable sex differences. Of forty-eight measurements and conditions reported upon the males showed a greater variability in 28 (58 per cent), the females in 9 (19 per cent), while in 11 (23 per cent) the variability in the two sexes was found to be nearly equal. The males varied more than the females especially in:

* Normal boundaries less definite than with other measurements.

Breadth of head	Nasal breadth and especially nasal index
Cephalic index	Ear dimensions and especially ear index
Height of forehead	Hand breadth
Gonio-frontal index	

The variability was about equal in the two sexes in:

Head height	Width of mouth
Height sitting-stature index	Breadth of chest
Cephalic module-stature index	Chest index
Mean head height index	Hand length
Morphological facial index	Foot breadth
Nascl index	

The females varied more than the males in:

Height of trunk	Breadth of face
Length of lower limbs	Physiognomic facial index
Weight	Muscular strength, especially traction

Much of this is fairly intelligible and will be of value in future comparisons. But there are some things shown above that for the present escape understanding and constitute an incentive to further research.

Even more important conditions, however, were found in this connection in racial comparisons. An example may be found in data on the length of the thigh bone, which are now being prepared for publication. There are four well represented racial groups in the material and their $R : A^2$ coefficients of variation appear as follows:

THE BICONDYLAR LENGTH OF THE FEMUR

	Specimens	Male	Specimens	Female
U. S. Whites, misc.	(580)	28.4	(200)	25.5
Old Peruvian	(1,000)	27.2	(750)	25.5
North American Indian, misc.	(622)	30.3	(250)	26.7
Alaskan Eskimo	(217)	26.0	(159)	26.4

The results are astonishingly close; and similar remarkable relations in variability were encountered by me with other measurements and determinations. This speaks very strongly for the unity of the human species. It is inconceivable that such resemblances in the males and practically identities in

the females, as presented by the above table, could be shown even by a group of distinct subspecies. Normal variation in man, one of the basic conditions of his being, is evidently also a valuable criterion as to his unity, and a proof of neither any profound nor geologically very long segregations within this unity.

Nor is this all the importance of the new light that begins to issue from these latest studies into man's constitution. There are other indications. These studies promise to furnish eventually a measure for the varied functional effects on the body or its parts, in different human groups and races, suggesting that it may sometime become possible to isolate and gauge the basic or inherent variability of any feature or organ and thus secure a most valuable means for future comparisons in bio-anthropological studies.

The general meaning of all this is that new ways are opening for man's understanding of himself, and that there is appearing a great new and highly promising field for anthropological and related researches of the future.

THE GENERA CORDYCEPS AND OPHIOCORDYCEPS IN MICHIGAN *

EDWIN B. MAINS

DURING the past two years a number of very interesting collections of Cordyceps have been obtained from various localities in Michigan. Two of these have proven to be undescribed species. In this paper it is proposed to discuss the species collected and briefly review others which have been reported in the state.

In the study of the various collections a number of questions arose regarding the identity of several species, necessitating an examination of authentic specimens. The writer wishes to express his appreciation to Dr. David H. Linder and Mrs. L. W. Riddle of the Farlow Herbarium, Harvard University, Dr. H. D. House of the New York State Museum and Professor Don Benedict of the Philadelphia Academy of Natural Sciences for the opportunities afforded for a study of such material. Thanks are also due Mr. T. Petch for helpful suggestions and criticisms.

CORDYCEPS

The genus Cordyceps as considered here includes those species with cylindrical asci and long, filiform, multiseptate ascospores which usually soon break up into segments. Petch (23) will be followed in considering that the species with clavate asci and fusoid ascospores should be placed in a separate genus which he has named Ophiocordyceps.

CORDYCEPS FORMICIVORA Schroeter. This curious species has been reported by Povah (20). He obtained one specimen on an ant at Rock River, Michigan, in 1927. This is apparently the first report for North America. Povah, however,

* Papers from the Department of Botany and the Herbarium of the University of Michigan, No. 469.

states that Professor Thaxter had collected it on several occasions at York, Maine.

CORDYCEPS GRACILIS Mont. & Dur., Douglas Lake, June 8, 1932, A. H. Smith (3296), Plate 3, *A-B*; Big Garlic River, Marquette, Mich., June 15, 1933, E. B. Mains (33-354). Kauffman (5) in 1909 collected this species from Detroit. Smith's collection is apparently on a lepidopterous larva. The host of the Marquette specimen was not collected. The Detroit collection obviously is from a larva of a beetle (a species of Elateridae according to F. M. Gaige).

This species has been known in this country and England under the name of *Cordyceps entomorrhiza* (Dicks.) Link. Lloyd (9, 11, 12, 15, 16, 17) a number of years ago pointed out that this is erroneous and that the species is *Cordyceps gracilis*. Petch (24) has recently reported the same conclusions. Dickson (3) applied the name *C. entomorrhiza* to a species which was later named *Torrubia cinerea* by the Tulasnes (27). *Cordyceps entomorrhiza*, according to Lloyd and Petch, is usually reported as *Cordyceps cinerea* (Tul.) Sacc. on the continent of Europe. *C. entomorrhiza* is a gray plant with prominent protruding perithecia (Plate 3, *C*) while *C. gracilis* is orange with only the ostioles slightly projecting out of the stromata (Plate III, *B*).

Cordyceps gracilis is apparently rare in North America. The species under the name of *C. entomorrhiza* is listed by Ellis and Everhart (4) and Seaver (25) only from South Carolina. Lloyd (15), however, concluded that Ravenel's collection upon which Ellis and Seaver based this record is probably *C. ophioglossoides*. Lloyd (15) in 1920 stated that only two collections were known to him from the United States, one made in New York by Peck and the other developed at the New York Botanical Garden from material sent from Indiana.

Lloyd reports (15) that all specimens of *Cordyceps gracilis* examined by him were on lepidopterous larvae, and that the true *C. entomorrhiza* (*C. cinerea*), as far as known, occurs only on larvae of species of *Carabus*. The specimen reported by

Kauffman (5) from Detroit is certainly not the latter species. It appears to be *C. gracilis* although it is on the larva of a beetle. It has the orange-yellow color and the deeply embedded perithecia of that species. Unfortunately it is immature and the ascospores are poorly differentiated.

CORDYCEPS HERCULEA (Schw.) Sacc. Apparently one collection of this species has been reported from Michigan (Kauffman, 7). This was obtained by the writer in Cascade Glen near Ann Arbor, August 7, 1915. It had developed from the larva of a June beetle. The species is more frequent southward.

Lloyd (10) has questioned the use of Schweinitz's name for the fungus which is commonly known as *Cordyceps herculea*. He states that an examination of "*Sphaeria herculea*" in the Schweinitz herbarium shows that it is a gasteromycete, *Cauloglossum transversarium*. Curiously enough, Coker and Couch (2) under *Rhopalogaster transversarium* states that "the specimen labeled *Cauloglossum transversarium* in the Philadelphia Herbarium (Ravenel, Fungi Car. Exs. 79) is not this species but *Sphaeria herculea* Schw. according to Ravenel." Lloyd (13) reaches the conclusion that *Cordyceps Melolonthae* (Tul.) Sacc. was described from an immature specimen of the fungus under discussion. Also after a study of the specimen of *Cordyceps insignis* at Kew he (10) decided that the latter also was the same plant. Seaver (25) places *C. Melolonthae* as a synonym of *Cordyceps acicularis* with a query, but recognizes *C. insignis* as a distinct species. Massee (18) recognizes all three as valid species.

Further study of type collections is necessary before final conclusions can be reached. With the exception of Lloyd, apparently the names *C. Melolonthae* and *C. insignis* have been applied to but few collections. Even if Lloyd is right in his conclusions, the desirability of a change of the name for the species is still questionable. If usage, a criterion which Lloyd emphasized so much, should determine the question the name *Cordyceps herculea* should stand.

Cordyceps michiganensis n.sp. Stromatibus 3-9, 1-2.5 cm. longis, ochraceo-fuscis, gracilibus, terminantibus in acuminatis sterilibus apicibus 0.5-5.0 mm. longis; peritheciis superficialibus, liberis, ovoideis, 324-420 \times 264-336 μ ; ascis cylindricis, 130-200 \times 4-8 μ ; ascosporis filiformibus, in articulos 24-50 \times 1-2 μ mox fragmentibus.

Ex larvis scarabaeorum. Specimen typicum in Herbario Michiganense conservatum. E. B. et E. E. Mains n. 32-391, prope Rock River, Michigan, August 28, 1932.

Stromata 3-9, "ochraceous-tawny."¹ slender, 1.0-2.5 cm. long, the apex sterile, acuminate, 0.5-5.0 mm. long, the fertile portion 3-5 mm. "hazel"; perithecia superficial, free, ovoid, 324-420 \times 264-336 μ ; asci cylindric, 130-200 \times 4-8 μ ; ascospores colorless, filiform, lying parallel in the ascus and about equaling it in length, breaking up into segments, 24-50 \times 1-2 μ (Plate I).

On larvae of a beetle in rotten logs, Rock River, Mich., Aug. 19, 1932, E. B. Mains (32-148), E. B. & E. E. Mains, Aug. 28, 1932 (32-391, type); Lakeland, Mich., July 1, 1933, E. B. Mains & A. H. Smith (33-379); George Reserve, Pinckney, Mich., July 25, 1933, E. B. Mains (33-403).

This species is characterized by its many, short, slender stromata, superficial, free perithecia and sterile apices. The delicate sterile apices appear to be a constant character since they occur on all the specimens. It differs from *Cordyceps superficialis* Peck in smaller, more numerous stromata, smaller perithecia and asci, and in the more yellow color. It is a dainty, little species, easily overlooked since the delicate stromata are not conspicuous. All the collections have been made in wet places either in rotted wood, in sphagnum or in swampy areas.

CORDYCEPS MILITARIS Link. Wagner's Falls, Munising, Mich., Aug. 25, 1932, E. B. Mains (32-323); Rock River, Aug. 27, 1932, E. B. and E. E. Mains (32-384); E. B. Mains (32-385); Aug. 29, 1932, E. B. Mains (32-419); Miner's Falls,

¹ All color names within quotation marks are taken from Ridgway, R., Color Standards and Nomenclature.

Munising, Aug. 30, 1932, E. B. Mains (32-434); Harbor Springs, Aug. 18, 1933, E. B. Mains and A. H. Smith (33-428).

Except for collection 32-384, only single specimens were obtained. The fifteen specimens of collection 32-348 were found scattered along a heavily wooded hillside sloping down to Au Train River. In all but one of the specimens the stromata arose from naked pupae. In collection 32-419, the pupa was inclosed in a cocoon.

This is probably the most common species of the genus for North America. It has apparently not often been reported for Michigan. Povah has listed it from Vermilion (19), Rock River (20), and Isle Royal (21).

The perithecia are sometimes described as superficial. Petch (24) has pointed out that they are at first immersed in a bysoid layer of hyphae. As the stroma dries this layer shrinks into a thinner layer, leaving the perithecia more prominent. If the perithecia are far apart this may even be broken up resulting in apparently free and superficial perithecia.

CORDYCEPS OPHIOGLOSSOIDES Link. This species has been collected a number of times in the vicinity of Ann Arbor on *Elaphomyces*. There are collections in the University Herbarium made by C. H. Kauffman, Aug. 8, 1921; L. A. Cannon, Aug. 2, 1923; E. B. Mains, Aug. 7, 1915; C. H. Kauffman, Aug. 14, 1915; A. H. Smith, Sept. 1, 1932 (32-325). It will probably be found in other localities of the state when search is made during the proper season.

CORDYCEPS STYLOPHORA Berk. & Br. This species has been collected only once in Michigan, by G. H. Hicks, April, 1892, near Michigan State College at East Lansing (Longyear 8, Ellis & Everhart 4). The asci are cylindrical and the spores filiform. The perithecia are embedded, only the ostiole showing (Plate II, *F*). The fertile portion is a definite cylindrical enlargement located in the mid-portion of the stromata leaving long sterile apices (Plate II, *E*). Apparently the only other report is the type collected by Ravenel in South Carolina (Berkeley 1).

CORDYCEPS SUPERFICIALIS (Peck) Sacc. This species has been reported for Michigan by Kauffman (6) from South Haven, July 1, 1910. Unfortunately this collection cannot be located and probably was lost in a fire which destroyed a number of rare specimens which Dr. Kauffman had in his office in 1913. The species was described by Peck (22) as *Torrubia superficialis* from a collection made in August (1874) at Northville, N. Y., on buried larvae under hemlock.

Peck in his discussion states that it is "related to and intermediate between *T. Ravenelii* and *T. carolinensis*." Seaver (25) apparently, on the basis of this statement, placed it with a query as a synonym of *Cordyceps acicularis* (*C. carolinensis*). Petch (24) has recently concluded that *C. Ravenelii* and *C. acicularis* are variations of the same species and on account of the clavate asci and fusoid ascospores has placed the species in *Ophiocordyceps* under the name *O. acicularis* including *Cordyceps superficialis* as a synonym.

Through the kindness of H. D. House, it has been possible to study the type of *Cordyceps superficialis* in the New York State Museum at Albany. This consists of two stromata glued on paper (Plate II, *A*). A study of these indicates that the species is valid. Since the original account does not give adequate information on a number of important points the following description has been prepared from the type collection.

Stromata "vinaceous-buff," 2.5 and 3.5 cm. long, 1.5-2 mm. in diameter, the stipes 10 and 15 mm. long, fertile portions 15 mm., one with sterile acuminate apex, 5 mm. long, the other with apex apparently broken off; perithecia superficial, free, somewhat crowded, "army-brown," ovoid, 456-660 \times 450-564 μ , some with apex collapsed or broken off; asci cylindric, 170-230 \times 7-9 μ ; ascospores hyaline, filiform, probably about equaling the asci in length, breaking into segments 14-30 \times 1.5-2 μ (Plate II, *A-D*).

Only a fragment of the host adheres to one stroma. The collapsed and empty perithecia indicate that the specimens were old when they were collected.

The cylindric asci and filiform ascospores separate this species without question from *Ophiocordyceps acicularis*.

OPHIOCORDYCEPS

This genus has been proposed by Petch (23) for those species of *Cordyceps* having more or less clavate asci and fusoid ascospores. The ascospores, instead of lying parallel in the ascus, overlap each other. The spores do not generally divide into segments at the septa. The following two species have been collected in Michigan.

OPHIOCORDYCEPS CLAVULATA (Schw.) Petch. Chatham, June 17, 1933, E. B. & E. E. Mains (33-365); Au Train Falls, Forest Lake, June 17, 1933, E. B. & E. E. Mains (33-368), on scale insects on maple (Plate 3, *D-E*); also East Lansing, Sept. 15, 1891, W. J. Beal on *Ilex verticellata*; Ann Arbor, Nov. 1916, Hartwell, Oct. 1920, Lee Bonar on ash. It has been reported by Longyear (8) and Povah (21) has collected it on Isle Royale.

Most of the Michigan collections are very meager. However, it was very abundant west of Munising in 1933, especially at Au Train Falls, and was collected in quantity. According to Lloyd (14) the species is rare in Europe.

The species has been known as *Cordyceps clavulata* Schw. It has fusoid ascospores $42-56 \times 2.5-3.5 \mu$, overlapping in the clavate ascus. Petch (24) consequently has recently transferred the species to *Ophiocordyceps*.

***Ophiocordyceps macularis* n.sp.** Stromatibus 1-2, ochraceo-carneis vel carneo-alutaceis, 2-3.5 cm. longis, 1 mm. crassis, acuminatis; peritheciis superficialibus, plusminusve in irregularibus massis aggregatis, ovoideis, $290-320 \times 190-220 \mu$; ascis anguste clavatis, $150-180 \times 6-8 \mu$; ascosporis anguste fusoideis, $60-90 \times 2-3 \mu$, multi-septatis, cellulis 6-14 μ longis.

Ex larvis scarabaeorum. Specimen typicum in Herbario Michiganense conservatum, E. B. Mains et A. H. Smith n. 33-453 prope Harbor Springs, Michigan, August 20, 1933.

Stromata 1 or 2, "light ochraceous-salmon" or "vinaceous fawn," 2-3.5 cm. long, 1 mm. diameter, acuminate; perithecia superficial, more or less grouped in irregular patches, crowded, with a slight subiculum, ovoid, $290-320 \times 190-220 \mu$, asci narrowly clavate, $150-180 \times 6-8 \mu$; ascospores narrowly fusoid, $60-90 \times 2-3 \mu$, obscurely multiseptate, the cells 6-14 μ long.

From larvae of a beetle in moss covered logs of maple. Harbor Springs, Mich., Aug. 20, 1933, E. B. Mains & A. H. Smith (33-453, type) (Plate IV).

The narrow clavate asci and the relatively short, fusoid overlapping ascospores definitely place this species in the genus *Ophiocordyceps*. Fourteen specimens were collected. These were found scattered along mossy, decaying, maple logs, arising from buried larvae of a species of beetle. The perithecia are crowded into irregular groups of various sizes or occasionally somewhat spirally arranged (Plate IV, D-F). The perithecia are superficial except for a slight subiculum.

This species resembles *Ophiocordyceps acicularis* (Berkeley 1) to some extent. The specimens of this species (Rav. Fungi Car. IV, 29) which have been studied in the Farlow Herbarium and from the Academy of Natural Science at Philadelphia have only sterile stromata. These are very long and slender, measuring 4.5-7.0 cm. long and 0.5 mm. wide. Petch (24), from material at Kew, has recently described the asci as narrowly clavate, $260-290 \times 7-10 \mu$, and the ascospores as linear-clavate, $150-240 \times 3-4 \mu$, multiseptate, with cells 4-6 μ long. *Ophiocordyceps macularis*, therefore, differs from *O. acicularis* in shorter more robust stromata, irregular distribution of perithecia and smaller asci and ascospores.

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PLATE I



PLATE I. *Cordyceps michiganensis* n.sp. A. Stromata from two buried larvae ($\times 1$). B. Stromata enlarged ($\times 2$), note superficial perithecia and sterile apices. C. Portion of cylindrical hyaline asci containing filiform ascospores ($\times 385$). D. Superficial perithecia ($\times 46$). E. Upper portion of a stroma enlarged ($\times 12$) showing superficial scattered perithecia and sterile apex.

PLATE II

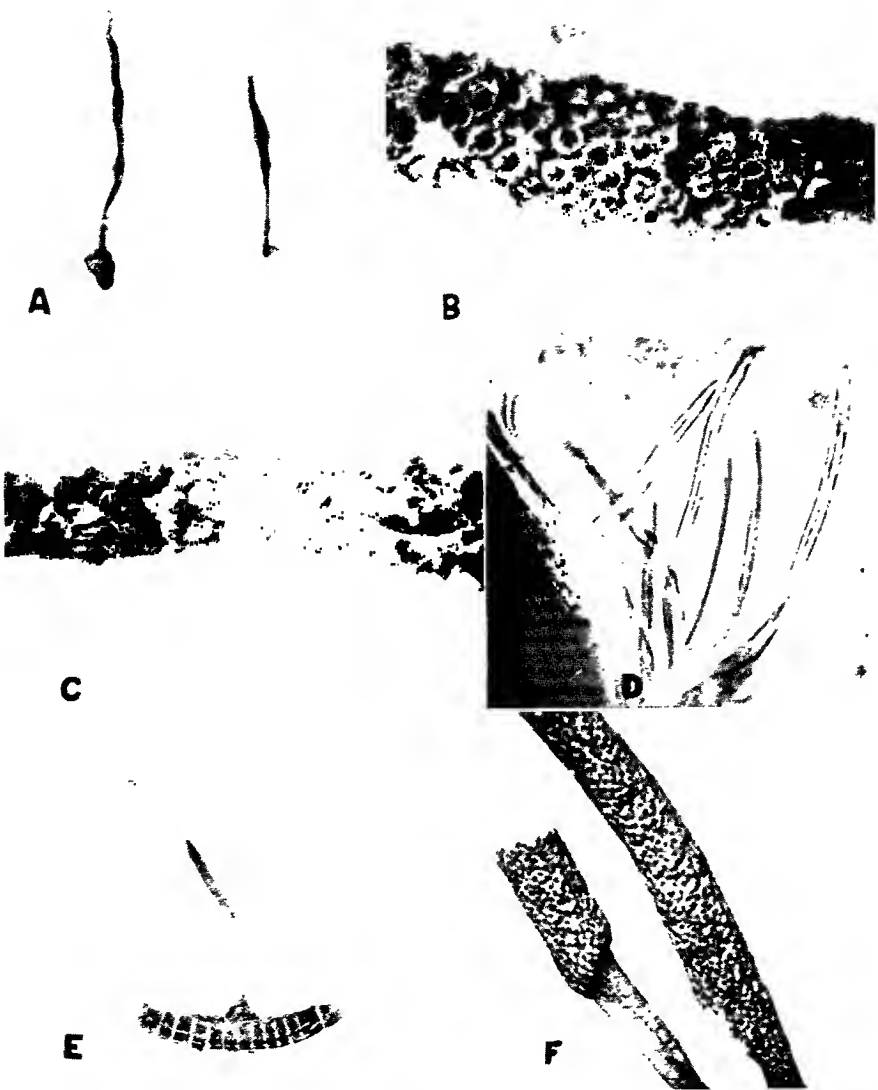


PLATE 2. *Cordyceps superficialis*. *A*. The two stromata of the type collection ($\times 1$). *B* and *C*. Portion of each stroma enlarged ($\times 10$) showing superficial perithecia. *D*. Portion of hyaline cylindrical asci containing filiform ascospores ($\times 385$).

Cordyceps stylopiora. *E*. Stroma with long sterile apex ($\times 2$), Ravenel Fungi Car. V: 49. *F*. Portion of stromata from collection by G. H. Hicks from near East Lansing, only ostioles of embedded perithecia projecting above stroma ($\times 65$).



PLATE III

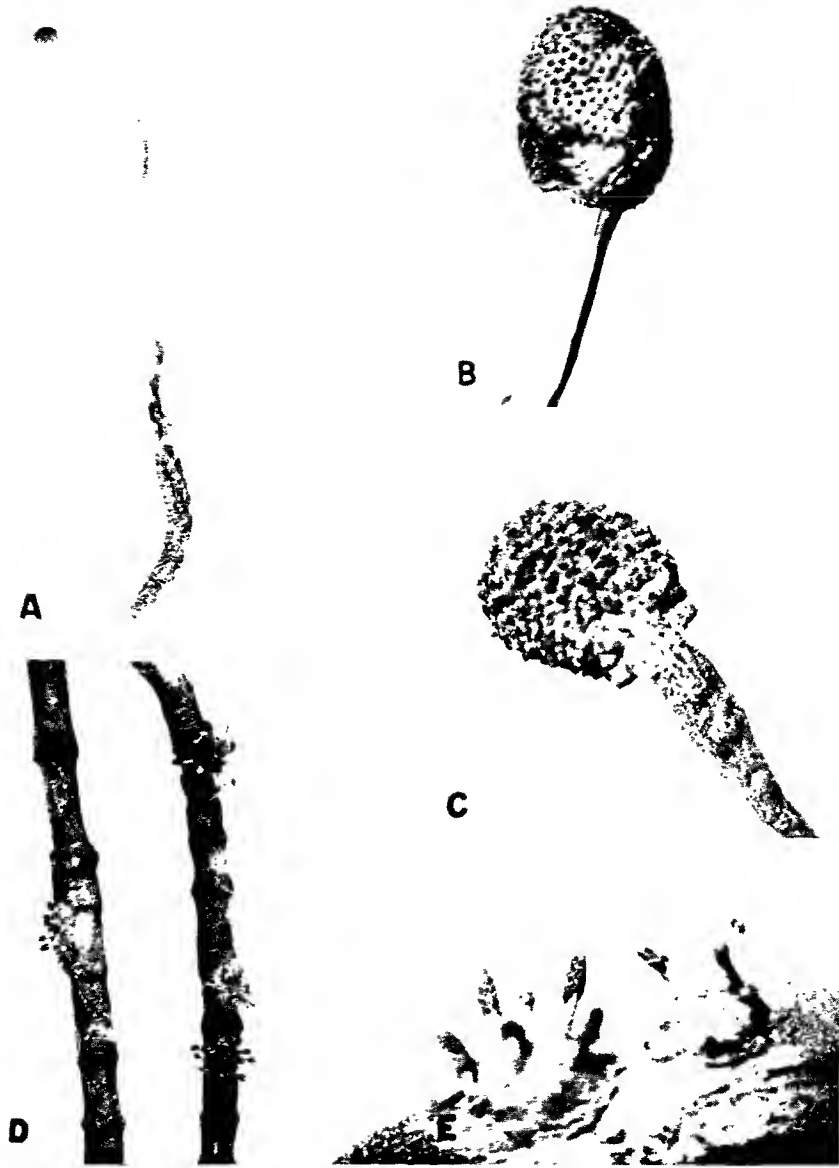


PLATE 3. *Cordyceps gracilis*. A. Specimen collected near Douglas Lake, Mich. ($\times 1$). B. Capitulum stroma of the same enlarged ($\times 8$), only ostioles of the embedded perithecia projecting.

Cordyceps enomorphiza. C. Portion of capitulum stroma (Rehm Ascomyceten 1288) showing projecting perithecia ($\times 8$).

Ophiocordyceps clavulata. D. Specimen from Lake Forest, Mich., showing stromata arising from scale insects ($\times 2$). E. Same enlarged ($\times 6.5$).

PLATE IV



PLATE 4. *Ophiocordyceps n. sp.* A. Stromata from several buried larvae ($\times 1$). B. Two stromata enlarged ($\times 3$) showing irregular grouping of perithecia. C. Clavate ascus with a fusoid ascospore adhering ($\times 385$). D, E, F. Showing irregular grouping of perithecia ($\times 10$).

THE THIRTY-NINE DISTINCT LINES OF PROBOSCIDEAN DESCENT, AND THEIR MIGRATION INTO ALL PARTS OF THE WORLD EXCEPT AUSTRALIA

HENRY FAIRFIELD OSBORN

(Read by title April 21, 1934)

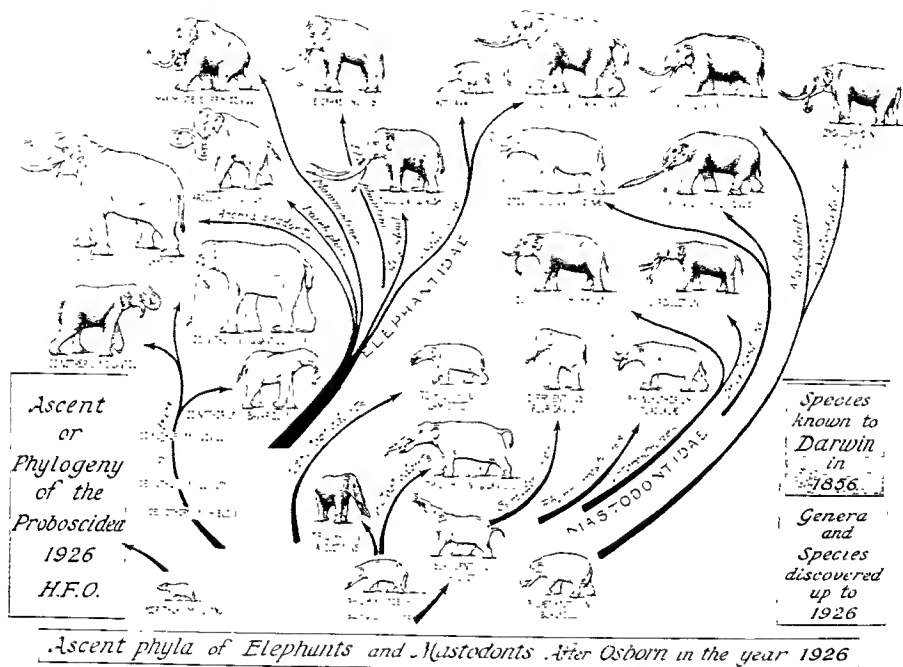
It is interesting to remark in the course of twenty-seven years of research, that the first communication on the Proboscidea by the present author was presented before the American Philosophical Society at the Annual Meeting April 24, 1908.

The evolution of the Proboscidea as now observed constitutes a veritable *revolution* not only in zoology but in biology. It affords the first actual and positive evidence as to the origin of species and reveals that evolution is a dual rather than a single process including the hitherto unrecognized creative principle which we call *aristogenesis*, as well as the older modifying principle which has been recognized as *evolution* for the last 2,500 years. These inductions are fully set forth and discussed in my eleventh and twelfth contributions¹ to the origin of species series. This is the thirty-first of a series of contributions on our present knowledge of the origin, evolution and migration of the Proboscidea, an order of mammals which in the popular mind includes only two genera, the mastodon and elephant.

As displayed in Figs. 1 and 2, Charles Darwin in the year 1859 knew only ten species of elephantoid Proboscideans which were referred to the single genus *Elephas*. We have now, up to the year 1934, determined not less than ten to eleven well founded genera of elephantoids and ninety-seven species, a very large number still remaining to be discovered. Charles

¹ Osborn, Henry Fairfield, "Aristogenesis, the Creative Principle in the Origin of Species," *The American Naturalist*, May-June, 1934, Vol. LXVIII, No. 716, pp. 193-235. *Idem*, "Senescent Hypotheses as to the Nature and Causes of Evolution" (not yet published).

Darwin in the year 1859 knew only seven species of the genera *Mastodon* and *Deinotherium*; these two genera of the Proboscidea are now known to include twenty-three valid genera and not less than one hundred and eighty-two species, a very large number of species remaining to be discovered.



Ascent phyla of Elephants and Mastodonts after Osborn in the year 1926

FIG. 1. Proboscidean phyla or lines of descent, 1926. Shading: Species of *Elephas*, *Mastodon* and *Deinotherium* known to Darwin up to the year 1856. Outlines: Genera and species known to Osborn up to the year 1926. In Darwin's time all the Mastodontoids were embraced within the single Cuvierian genus *Mastodon*. Darwin's *Mastodon* included only seven species in 1859 when he wrote his "Origin of Species"; it now includes twenty-three valid genera and not less than one hundred and eighty-two species, a very large number of species remaining to be discovered.

Even in the modern scientific mind, as shown in the learned articles² by Professor D. M. S. Watson of the University of London, the Proboscidea include only about seven outstanding genera and fourteen species succeeding each other as follows:

² Watson, D. M. S., "Proboscidea," *The Encyclopædia Britannica* (fourteenth edition), 1929, Vol. 18, pp. 541-543; *Idem*, "Mastodon," *op. cit.*, Vol. 15, pp. 42-43; *Idem*, "Elephant," *op. cit.*, Vol. 8, p. 349; *Idem*, "Mammoth," *op. cit.*, Vol. 14, pp. 736-737.

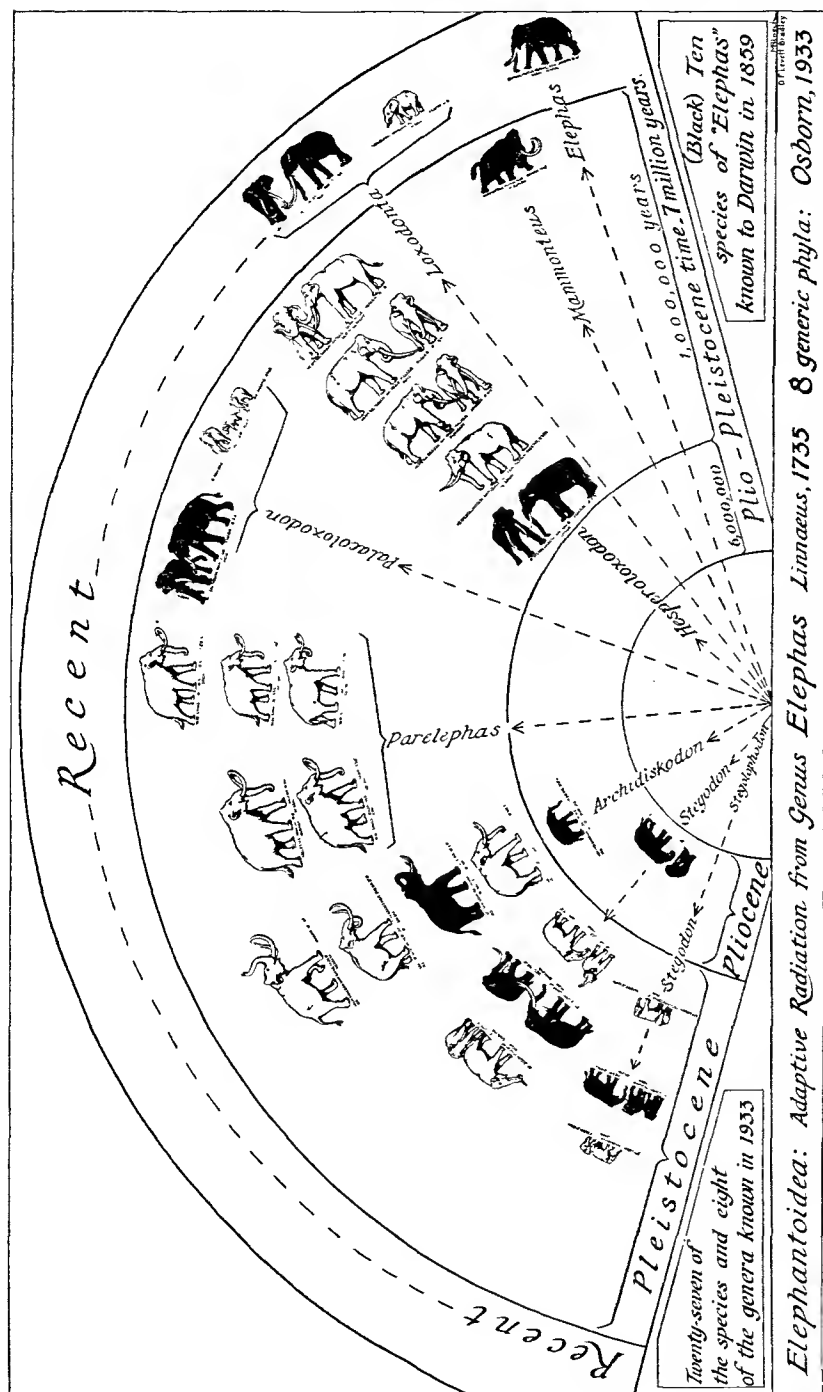


FIG. 2. Elephantoid phyla or lines of descent, 1933. Black: Ten species of the single genus *Elephas* known to Darwin in the year 1859. Outlines: Radiating lines of descent of the genera of Elephantoidae described in Osborn's memoir of 1934. In Darwin's time all the living and fossil Elephantoids were embraced within the Linnean genus *Elephas*. Darwin knew of ten species only, including Falconer's *Elephas ganeyi* (now *Stegodon*). We have now determined not less than ten well-founded genera of Elephantoids and ninety-seven (plus 14?) species, a very large number still remaining to be discovered.

Mæriotherium of Egypt, earliest known ancestral stage,
Palæomastodon of Egypt, a great advance on *Mæriotherium*,
Tetrabelodon angustidens of Europe, North Africa and Baluchistan, with an elephant-like body,
Tetrabelodon longirostris of Europe, a further mastodont stage,
Mastodon borsoni of Europe,
Mastodon americanus of America, the typical mastodont,
Deinotherium, a side branch of the mastodonts with an elephant-like body,
Stegodon of southern Asia, a transition to the elephant stage,
Elephas primigenius, the typical woolly mammoth,
Elephas antiquus, Pleistocene elephant of Eurasia,
Elephas imperator, the imperial mammoth of Nebraska,
Elephas hysudricus, a primitive elephant of India,
Elephas maximus, the Asiatic elephant,
Elephas (Loxodon) africanus, the African elephant.

In the present communication we may present a convenient synopsis of the genera and species of the more primitive divisions, namely, *Mæritheres*, *Deinotheres* and *Mastodonts* accompanied by the chart (Figure 3), leaving the synopsis of the higher elephantoid division for a future communication.

The four major divisions of the Proboscidea are the following:

Order: **PROBOSCIDEA**

Suborder: **MÆRITHERIOIDEA**

Primitive aquatic Proboscideans, with hippopotamus-like, or Sirenian-like bodies.

<i>Mæritheres</i>	{	I. MÆRITHERIUM	
		5) <i>Mæriotherium andrewsi</i> Schlosser, 1911, Egypt...	Lower Oligocene
		4) <i>Mæriotherium trigodon</i> Andrews, 1901, Egypt...	" "
		3) <i>Mæriotherium lyonsi</i> Andrews, 1901, Egypt....	Upper Eocene
		2) <i>Mæriotherium gracile</i> Andrews, 1902, Egypt...	" "
		1) <i>Mæriotherium ancestrale</i> Petronievics, 1923, Egypt.....	" "

Suborder: DEINOTHERIOIDEA

With very large down-turned inferior tusks, no superior tusks; with elephantine bodies.

- | | | | | |
|-------------|---|------------------|--|-----------------|
| Deinotheres | { | II. DEINOTHERIUM | | |
| | | 5) | <i>Deinotherium gigantissimum</i> Stefanescu, 1892,
Rumania. | Middle Pliocene |
| | | 4) | <i>Deinotherium giganteum</i> Kaup, 1829, Germany. | Lower Pliocene |
| | | 3) | <i>Deinotherium bavaricum</i> von Meyer, 1831,
Bavaria. | Middle Miocene |
| | | 2) | <i>Deinotherium hungaricum</i> Éhik, 1929, Hungary. | Lower Miocene |
| | | 1) | <i>Deinotherium kobleyi</i> Andrews, 1911, Africa. ... | Lower Miocene |

Suborder: MASTODONTOIDEA

With superior tusks, with or without inferior tusks, typically with mastodontine bodies.

- | | | | | |
|-----------------------|---|---------------------|--|-----------------|
| Beak-jawed Mastodonts | { | III. RHYNCHOTHERIUM | | |
| | | 5) | <i>Rhynchotherium falconeri</i> Osborn, 1923, Texas . | Upper Pliocene |
| | | 4) | <i>Rhynchotherium shepardii edensis</i> Frick, 1921-
1926, California. | " " |
| | | 3) | <i>Rhynchotherium broxtoni</i> sp. nov. Mexico. | Middle Pliocene |
| | | 2) | <i>Rhynchotherium tlascalae</i> Osborn, 1918, Mexico. | " " |
| | | 1) | <i>Rhynchotherium spenceri</i> Fourtau, 1918, Egypt. | Middle Miocene |
| | | IV. BLICKOTHERIUM | | |
| | | 2) | <i>Blickotherium blicki</i> Frick, 1933, Honduras. | Pliocene |
| | | 1) | <i>Blickotherium euhypodon</i> Cope, 1884, Ne-
braska. | Lower Pliocene |
| | | V. AYBELODON | | |
| 3) | <i>Aybelodon hondurensis</i> Frick, 1933, Honduras. . | Upper Pliocene | | |

- Typical yoke-crested Mastodonts
- VI. ZYGLOPHODON
- 5) *Zygodon borsoni* Hays, 1834, Italy..... Upper Pliocene
- VII. TURICIUS
- 6) *Turicius wahlheimensis* Klahn, 1922, Germany. Pliocene
 5) *Turicius virgatus* von Meyer, 1867, Germany " "
 4) *Turicius aticus* Wagner, 1857, Greece..... Lower Pliocene
 3) *Turicius turicensis* Schinz, 1824, Switzerland... Upper Miocene
 2) *Turicius turicensis simorreus* Osborn, 1926,
 France..... Middle Miocene
 1) *Turicius tapiroides* Cuvier, 1806, France..... Lower Miocene
- VIII. MASTODON, MIOMASTODON, PALÆOMASTODON
- 6) *Mastodon americanus* [Elephas americanus Kerr,
 1792] Kentucky..... Pleistocene
 4) *Miomastodon merriami* Osborn, 1921, Nevada.. Middle Miocene
 3) *Palæomastodon beadnelli* Andrews, 1901, Egypt Lower Oligocene
 2) *Palæomastodon intermedius* Matsumoto, 1922,
 Egypt " "
 1) *Palæomastodon parvus* Andrews, 1905, Egypt... " "
- IX. PLIOMASTODON
- 5) *Pliomastodon vexillarius* Matthew, 1930, Cali-
 fornia..... Late Pliocene

X. TRILOPHODON and PHOMIA

- | | | |
|-----|--|---------------------|
| 12) | <i>Trilophodon angustilens gallardi</i> Osborn, 1929,
France | Lower Pliocene |
| 11) | <i>Trilophodon marignathus</i> Pilgrini, 1913, India | Upper Miocene |
| 10) | <i>Trilophodon chinensis</i> Pilgrim-Osborn, 1913,
1932, India | " " |
| 9) | <i>Trilophodon angustilens austro-germanicus</i> Weg-
ner, 1908, 1913 Germany | Middle Miocene |
| 8) | <i>Trilophodon angustilens</i> Cuvier, 1806, 1817,
France | " " |
| 7) | <i>Trilophodon palaeindicus</i> Lydekker, 1884, India | " " |
| 6) | <i>Trilophodon pontieriensis</i> Mayet-Fourtau, 1918
France | Lower Miocene |
| 5) | <i>Trilophodon cooperi</i> Osborn, 1932, Baluchistan | Lower Miocene |
| 4) | <i>Phomia pygmaeus</i> Depéret, 1897, Algeria | Upper (?) Oligocene |

XI. TRILOPHODON and TATABELODON—Oblique Tuskers

- | | | |
|----|---|-----------------|
| 8) | <i>Trilophodon giganteus</i> Osborn, 1921, South
Dakota | Lower Pliocene |
| 7) | <i>Trilophodon phippsi</i> Cook, 1923, Nebraska | Middle Pliocene |
| 6) | <i>Trilophodon (Tatabelodon) gregorii</i> Frick, 1933,
Nebraska | Mio-Pliocene |
| 5) | <i>Trilophodon (Tatabelodon) riograndensis</i> Frick,
1933, New Mexico | " " |

XII. TRILOPHODON and PHOMIA—Prod Tuskers

- | | | |
|----|---|----------------|
| 9) | <i>Trilophodon (Megabelodon) lulli</i> Barbour, 1914,
1917, Nebraska | Upper Pliocene |
| 8) | <i>Trilophodon abeli</i> Barbour, 1925, Nebraska | Lower Pliocene |
| 7) | <i>Trilophodon pojoaqueensis</i> Frick, 1926, New
Mexico | Lower Pliocene |
| 6) | <i>Trilophodon (Genomastodon) osborni</i> Barbour,
1916, Nebraska | Lower Pliocene |
| 5) | <i>Trilophodon cruziensis</i> Frick, 1933, New Mexico | Upper Miocene |
| 4) | <i>Trilophodon fricki</i> Peterson, 1928, Colorado | Middle Miocene |

XIII. AMEBELODON and PHOMIA—Shovel Tuskers

- | | | |
|----|--|--------------------------|
| 8) | <i>Amebelodon fricki</i> Barbour, 1927, Nebraska | Upper Pliocene |
| 7) | <i>Amebelodon (Tril.) hicksi</i> Cook, 1922, Colorado | Middle-Lower
Pliocene |
| 6) | <i>Amebelodon (Tril.) paladentatus</i> Cook, 1922,
Colorado | Lower Pliocene |
| 5) | <i>Phomia osborni</i> Matsumoto, 1922, Egypt | Upper Oligocene |
| 3) | <i>Phomia serridens</i> Andrews and Beadnell, 1902,
Egypt | Lower Oligocene |
| 2) | <i>Phomia wintoni</i> Andrews, 1905, Egypt | " " |
| 1) | <i>Phomia minor</i> Andrews, 1904, Egypt | " " |

- Medium-jawed Tetralophodonts
- XIV. EURASIATIC TETRALOPHODONTS
- 6) *Tetralophodon grandincisivus* Schlesinger, 1917,
Persia..... Middle Pliocene
 - 5) *Tetralophodon punjabiensis* Lydekker, 1886,
India..... " "
 - 4) *Tetralophodon (Lydekkeria) falconeri* Lydekker,
1877, India..... " "
 - 3) *Tetralophodon bumiajuensis* van der Maarel,
1932, Java..... Pliocene
 - 2) *Tetralophodon longirostris* Kaup, 1832, Germany Lower Pliocene
 - 1) *Tetralophodon (Lydekkeria) sinensis* Koken,
1885, China..... Uppermost Miocene
- XV. AMERICAN TETRALOPHODONTS
- 5) *Tetralophodon (Morrillia) barbouri* Osborn, 1921,
Nebraska..... Upper Pliocene
 - 4) *Tetralophodon brazosius* Hay, 1923, Texas..... ?Pliocene
 - 3) *Tetralophodon elegans* Hay, 1917, Kansas..... ?Lower Pliocene
 - 2) *Tetralophodon campester* Cope, 1878, Kansas... Lower Pliocene
 - 1) *Tetralophodon fricki* sp. nov. Texas..... Lower Pliocene

XVI. SERBELODON and TROBELODON

- 3) *Serbelodon burnhami* Osborn, 1933, California . . . Upper Pliocene
- 2) *Serbelodon taricauensis* Frick, 1933, Nebraska . . . Lower Pliocene
- 1) *Trobelodon tabori* Frick, 1933, New Mexico . . . Mio-Pliocene

XVII. SERRIDENTINUS PRODUCTUS Group

- 8) *Serridentinus guatemalensis* Osborn, 1926, Central America Upper Pliocene
- 7) *Serridentinus progressus* Osborn, 1923, Nebraska Upper Middle Pliocene
- 6) *Serridentinus laotensis* Osborn, 1929, India Middle Pliocene
- 5) *Serridentinus productus* Cope, 1875, New Mexico Upper Miocene
- 4) *Serridentinus gobiensis* Osborn and Granger, 1932, Inner Mongolia Mio-Pliocene
- 3) *Serridentinus natchinjiangensis* Osborn, 1929, India Upper Miocene
- 2) *Serridentinus prochinjiangensis* Osborn, 1929, India " "
- 1) *Serridentinus subaprioides* Schlesinger, 1917, Austria Lower Middle Miocene

XVIII. SERRIDENTINUS SERRIDENS Group—Sharp Crested

- 6) *Serridentinus praecursor* Cope, 1892, Texas Upper Pliocene
- 5) *Serridentinus serridens cimarronis* Cope, 1893, Texas Lower Pliocene
- 4) *Serridentinus anguirivalis* Osborn, 1926, Nebraska " "
- 3) *Serridentinus serridens* Cope, 1884, Texas " "
- 2) *Serridentinus proavus* Cope, 1873, Colorado Upper Miocene
- 1) *Serridentinus chinjensis* Osborn, 1929, India " "

XIX. OCALIENTINUS

- 5) *Ocalientinus* (Ser.) *florescens* Osborn, 1929, Mongolia Upper Pliocene
- 4) *Ocalientinus* (Ser.) *obliquidens* Osborn, 1926, South Carolina Middle "
- 3) *Ocalientinus* (Ser.) *floridanus* Leidy, 1886, Florida Lower Pliocene
- 2) *Ocalientinus* (Ser.) *republicanus* Osborn, 1926, Kansas " "
- 1) *Ocalientinus ojocaliensis* Frick, 1933, New Mexico Mio-Pliocene

XX. SERRIDENTINUS BROWNI Group—Blunt Crested

- 3) *Serridentinus browni* Osborn, 1926, India Upper Miocene
- 2) *Serridentinus mongoliensis* Osborn, 1924, Mongolia Middle Miocene
- 1) *Serridentinus annectens* Matsumoto, 1924, Japan Lower Miocene

XXI. PLATYBELODON

- 3) *Platybelodon barnumbrowni* Barbour, 1931, Nebraska Lower Middle Pliocene
- 2) *Platybelodon grangeri* Osborn, 1929, Mongolia Upper Miocene
- 1) *Platybelodon danovi* Borissiak, 1928, north Caucasus Lower Pliocene

Serridentine, serrate-toothed Mastodonts

Brevirostrinae, short-jawed or torsion toothed	XXII. PENTALOPHODON	
	5) <i>Pentalophodon falconeri</i> sp. nov., India.	Base of Pleistocene
	4) <i>Pentalophodon sizalensis</i> Cautley, 1836, India.	Upper Pliocene
	XXIII. ANANCUS	
	4) <i>Anancus falconeri</i> Osborn, 1926, England.	Uppermost Pliocene
	3) <i>Anancus arvernensis</i> Croizet and Jobert, 1828, France	Upper Pliocene
	2) <i>Anancus perimensis</i> Falconer and Cautley, 1847, India.	Middle Pliocene
	1) <i>Anancus properimensis</i> sp. nov. India.	Upper Miocene
	XXIV. SYNCONOLOPHUS	
	3) <i>Synconolophus dhokpathanensis</i> Osborn, 1929, India.	Middle Pliocene
	2) <i>Synconolophus hasnoti</i> Pilgrim, 1913, India.	" "
	1) <i>Synconolophus corrugatus</i> Pilgrim, 1913, India.	" "
Short-jawed Humboldtine Mastodonts	XXV. CUVIERONIUS and EUBELODON	
	3) <i>Cuvieronius superbus</i> Ameghino, 1888, Argentina	Upper Pleistocene
	2) <i>Cuvieronius platensis</i> Ameghino, 1888, Argentina.	Lower Pleistocene
	1) <i>Eubelodon morrilli</i> Barbour, 1914, Nebraska.	Middle Pliocene
	XXVI. CUVIERONIUS	
	5) <i>Cuvieronius postremus</i> Spillmann, 1928-1930, Ecuador.	Upper Pleistocene
	4) <i>Cuvieronius ayora</i> Spillmann, 1928-1931, Ecuador.	?Lower Pleistocene
	XXVII. STEGOMASTODON	
	4) <i>Stegomastodon afronia</i> Osborn, 1924, Iowa.	Lower Pleistocene
	3) <i>Stegomastodon arizonæ</i> Gidley, 1926, Arizona.	Uppermost Pliocene
	2) <i>Stegomastodon texanus</i> Osborn, 1924, Texas.	Upper Pliocene
	1) <i>Stegomastodon mirificus</i> Leidy, 1858, Nebraska.	" "
Medium-jawed Andean Mastodonts	XXVIII. CORDILLERION	
	2) <i>Cordillerion andium</i> Cuvier, 1806, 1824, Ecuador.	Pleistocene
	1) <i>Cordillerion edensis</i> Osborn, 1922, California.	Upper Pliocene
	XXIX. NOTIOMASTODON (Incertae sedis)	
	2) <i>Notiomastodon ornatus</i> Cabrera, 1929, Argentina.	Lower Pleistocene
	1) <i>Notiomastodon argentinus</i> Ameghino, 1888, Argentina.	Plio-Pleistocene

Suborder. ELEPHANTOIDEA

With superior tusks, abbreviated mandibles, long-limbed elephantine bodies, proboscis reaching the ground.

- I. *STEGOLOPHODON* Schlesinger, primitive Mio-Pliocene Stegodonts of Eurasia.
- II. *STEGODON* Falconer, with ridge plates relatively short, not ancestral to the elephants, an independent side line of southern Asia and China.
- III. *ARCHIDISKODON* Pohlig, with thick enamel ridge plates, migrating from southern Africa to Mexico.
- IV. *PAELEPHAS* Osborn, with enamel ridge plates of intermediate thickness characteristic of the north temperate zone of Eurasia and North America, migrating to French Guiana.
- V. *MAMMONTEUS* Camper, typical mammoth with excessively fine ridge plates characteristic of the entire Holarctic region of Eurasia and North America.

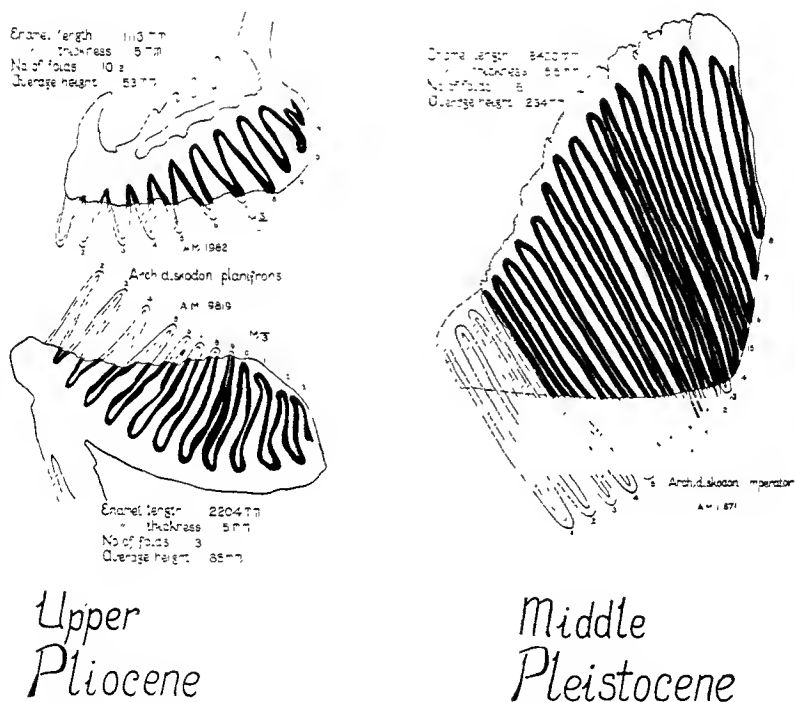


FIG. 4. Accelerated elephantine ridge crests in *Archidiskodon*. Intensely accelerated evolution of the ridge plates from the *Archidiskodon planifrons* of southern Eurasia into the *Archidiskodon imperator* of the United States and Mexico, all occurring from Upper Pliocene to Middle Pleistocene time. Length of enamel foldings:

Archidiskodon imperator of Texas = 8420 mm.

Archidiskodon planifrons of India = 2204

Archidiskodon planifrons of India = 1113

In these southern mammoths of Africa, Eurasia and North America, gigantic size is attained in a relatively short period of geologic time.

- VI. *HESPEROLOXODON* Osborn, a *Loxodontine* of western Eurasia and Africa, never reaching America.
- VII. *PALÆOLOXODON* Matsumoto, of eastern Eurasia and the Mediterranean Islands, never reaching America.
- VIII. *LOXODONTA* F. Cuvier, primitive surviving elephants of Africa.
- IX. *HYPSELEPHAS*, gen. nov., primitive elephants of India with elevated cranium.
- X. *PLATELEPHAS*, gen. nov., elephants of India with flattened cranium.
- XI. *ELEPHAS* Linnaeus, modernized elephants of India.

The author hopes to present the phylogeny and classification of the Elephantoidea to the American Philosophical Society at the Annual Meeting of 1935. The new results obtained in the author's researches on the Elephantoidea are no less revolutionary than those on the Mastodontoidea. Every month additions are being made to our knowledge. By far the most important discoveries still to be made are in the relatively unexplored continent of Africa where the British and Colonials in the south and in the equatorial regions, and the French in the northern portions are every year adding fresh proofs that the continent of Africa was the original home of the great order PROBOSCIDEA.

A REVISION OF THE AMERICAN APATEMYIDÆ AND THE
DESCRIPTION OF A NEW GENUS, *SINCLAIRELLA*, FROM
THE WHITE RIVER OLIGOCENE OF SOUTH DAKOTA

GLENN L. JEPSEN

(Read April 19, 1934)

CRUSHED fossil remains of several types of small mammals were collected from a limited area in the Oligocene Chadron formation ("Titanotherium beds") of the White River Badlands, South Dakota, by a Princeton Scott Fund Expedition in 1932. The numerous delicate microfaunal specimens were removed from the field in blocks of the soft clay matrix and subsequently worked out in the laboratory by Mr. John Clark during a period of two years. Among the unique types which he excavated under a microscope were the skull and jaws of a curiously specialized creature, immediately recognized as related to the "Plesiadapids," previously known from the Paleocene and Eocene formations of America and Europe. This new Oligocene genus and species is here named *Sinclairiella dakotensis* in approbation of Professor W. J. Sinclair's successful work with the White River faunas and sediments of South Dakota.

Most of the specimens which have been assigned to the peculiar mammalian group to which *Sinclairiella* belongs consist of jaw and skull fragments. Three family names (with various spellings) have been proposed to receive this assemblage: PLESIADAPIDÆ, APATEMYIDÆ, and CHIROMYIDÆ. In recent years these three family names have been regarded as synonymous, with the first having precedence. The incompleteness of the many generic types which compose the PLESIADAPIDÆ has made the family's ordinal assignment highly arbitrary. Different authors have suggested that the aggregation may be insectivores, or primates, or rodents, or

near the border line between the primates and the insectivores. These numerous opinions expressed in the literature about the PLESIADAPIDÆ have complicated the synonymy and homonymy. Careless references, typographical errors,¹ and incomplete or incorrect statements also add to the confusion.

Since the skull of *Sinclairella* is more complete than that of any "Plesiadapid" heretofore recorded, its study led to a reëxamination of all available American specimens assigned to the family, and to the conjecture that at least two, and possibly more, widely different families have been united under the name PLESIADAPIDÆ. This same idea has been expressed by Matthew,² Abel,³ Teilhard, Simpson,⁴ and others. The present revision merely attempts a clear separation of one closely related group, for which Matthew's family name APATEMYIDÆ is revived, from the other Plesiadapids.

As herein redefined, the APATEMYIDÆ have a unique and clearly delineated cluster of family characters which separates them from the also relimited PLESIADAPIDÆ. The geologic ranges of the two groups in America and Europe fortify these suggestions.

This work has been aided by new microscopic technique and the elucidation of certain critical observations by skiagrams. The encouraging counsel of Professors W. J. Sinclair and W. K. Gregory is greatly appreciated.

The following species, listed according to geologic occurrence, have been reviewed in preparation for the description of *Sinclairella*:

¹ See G. J. Ossenkopp, "Übersicht unserer derzeitigen Kenntnis von den fossilen niederen Primaten," *Ergebnisse der Anatomie und Entwicklungsgechichte*, 1925, Band 26, p. 474, where the lower dental formula of *Apatemys* is misprinted as 1-2-3-2, instead of 1-0-2-3. The upper tooth formula of *Ignacius* is given as 1-2-3-3, a change from Matthew's determination of ?-1-2-3.

² W. D. Matthew and W. Granger, "A Revision of the Lower Eocene Wasatch and Wind River Faunas," *Bull. Am. Mus. Nat. Hist.*, Vol. XXXIV, Art. XIV, 1915, p. 478.

³ O. Abel, *Die Stellung des Menschen im Rahmen der Wirbeltiere*, 1931, p. 288.

⁴ G. G. Simpson, "Paleocene and Lower Eocene Mammals of Europe," *Am. Mus. Nov.*, No. 354, June 5, 1929, p. 13.

AMERICAN TYPES ¹

Torrejón, Montana.

1. *Pronothodectes matthewi* Gidley

Torrejón, Wyoming.

2. *Plesiolestes problematicus* Jepsen

Tiffany, Colorado.

3. *Plesiadapis gidleyi* (Matthew)
4. *Labidolemur soricoides* Matthew and Granger
5. *Ignacius frugivorus* Matthew and Granger
6. *Phenacolemur* sp.²

Bear Creek, Montana.

7. *Labidolemur kayi* Simpson

Tiffany—Bear Creek equivalent, Wyoming

8. *Plesiadapis fordinatus* Jepsen
9. *Phenacolemur pagei* Jepsen

Clark Fork, Wyoming.

10. *Plesiadapis dubius* (Matthew)
11. *P. cookei* Jepsen

Gray Bull, Wyoming.

12. *Plesiadapis cookei* Jepsen
13. *Phenacolemur praecox* Matthew
14. *P. citatus* Matthew
15. *Teilhardella chardini* Jepsen

Lysite, Wyoming.

16. *Phenacolemur* (undescribed species)

Bridger, Wyoming.

17. *Apatemys bellus* Marsh
18. *A. bellulus* Marsh
19. *A. rodens* Troxell

¹ Certain fragmentary and questionable types are omitted.

² This is A. M. N. H. specimen No. 17405, consisting of a left M₃ in a jaw fragment and a loose left M₁ or M₂, hesitantly referred to *Labidolemur soricoides* by Matthew and Granger, and later believed by Simpson to belong to another genus. The holotype of *Ignacius frugivorus* is a fragmentary "upper jaw with C, P⁴-M³ and alveoli of remaining cheek teeth." Matthew and Granger suggested the possibility that *Labidolemur* might be the lower dentition of *Ignacius*. A more probable conception is that the upper teeth of *Labidolemur* have not yet been found or properly identified, and that *Ignacius* is the maxillary fragment of a *Phenacolemur*. *Phenacolemur* ranges from the Tiffany (and equivalents) to the Lysite and occurs most abundantly in the Gray Bull. Lower and uppers have never been found associated, but Matthew stated in 1915 "Although these molars (upper) are not associated with the lower teeth of *Phenacolemur*, they accord very well with the inferential construction of the upper teeth of that genus, from the character of the lower teeth, and there is no other known genus of the Lower Eocene to which they could belong." Subsequent discoveries of more complete upper tooth series and a flattened skull (Princeton No. 13028) serve to indicate the force of Matthew's logic.

20. *Trogolemur myodes* Matthew
21. *Uinasorex parvulus* Matthew
Uinta, Utah.
22. *Stehlinella Uintensis* Matthew
Chadron, South Dakota.
23. *Sinclairiella dakotensis*

EUROPEAN TYPES ¹

Thanetian (and equivalents).

1. *Plesiadapis tricuspidens* Gervais
(= *P. remensis* Lemoine = *P. gervaisi* Lemoine
= *P. gidleyi* Matthew)

2. *Chiromyoides* ² *campanicus* Stehlin

Sparnacian (and equivalents).

3. *Plesiadapis* ³ *daubrei* Lemoine
4. *P. orsmaelensis* Teilhard
5. *P. richardsoni* (Charlesworth)
6. *Eochiromys landenensis* Teilhard
7. ? *Heterohyus* sp.

Lutetian-Bartonian (and equivalents).

8. *Heterohyus quercyi* (Filhol)
(= *Heterochiromys gracilis* Stehlin)
9. *H. armatus* Gervais
(= *Heterochiromys fortis* Stehlin)
10. *H. europaeus* (Rütimyer)
11. *H. heufelderi* Heller
12. *H. nanus* Teilhard

¹ Synonymy is indicated where two or more European authors are in agreement.

² Teilhard, *Les Mammifères de l'Eocene Inferieur Francais*, 1921, p. 21, regards *Chiromyoides* as synonymous with *Plesiadapis*, but Abel, *Die Stellung des Menschen im Rahmen der Wirbeltiere*, 1931, p. 271, does not.

³ In order to avoid changing the generic name *Plesiadapis* Gervais, which dates only from 1877 but which has become commonly used in the literature, to *Platychoerops* Charlesworth which has precedence, originating in 1854, Simpson proposes to restrict the former to Thanetian (Paleocene) and the latter to Sparnacian (Eocene) types. This would be a convenient artifice, but *Plesiadapis cookei*, a large species closely related to the Sparnacian forms, comes from both sides of the Paleocene-Eocene boundary, from the Clark Fork and from the Gray Bull. If later work dictates the advisability of including the Clark Fork in the Eocene, the difficulty is still not avoided because *Plesiadapis dubius* also comes from the Clark Fork. Several American Eocene and Paleocene species of *Plesiadapis* intergrade between the features which Simpson listed as diagnostic of the two genera, and his suggestion is therefore regretfully unused here. Teilhard is followed in continuing to use the better-known name despite the rules of nomenclature.

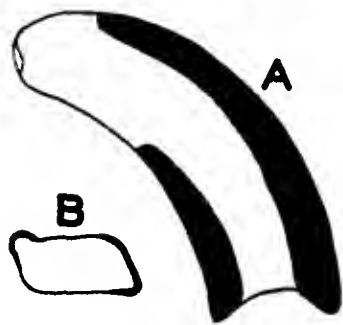
The relationships of Apatemyids and Plesiadapids will be discussed after the detailed description of *Sinclairella* which follows.

Sinclairella dakotensis, new genus and species.

Holotype.—Princeton No. 13585; a crushed skull lacking the right zygomatic arch, left P^1 and M^{1-2} , the crowns of I^2 on both sides, and the crown of the right I^1 ; left lower jaw lacking P_4 , the anterior part of P_3 and the tip of the coronoid process; badly crushed right lower jaw lacking P_{3-4} and M_1 . The condition of the enamel on the teeth indicates that the individual was a young adult.

Horizon and Locality.—Upper part of Chadron formation (about 11 feet below the limestone zone which is usually considered as the boundary between the Chadron and the Brule formations), near the head of the west fork of the east branch of Big Corral Draw, Washington County, about 13 miles SSW. of Scenic, South Dakota.

Characters.—Dental formula, $I \frac{2}{1} C \frac{0}{0} P \frac{2}{2} M \frac{3}{3}$ (or $I \frac{2}{1} C \frac{1}{0} P \frac{1}{2} M \frac{3}{3}$, probably the former).



TEXT FIG. 1. A. Outline of right I^1 of *Sinclairella*, $\times 3$. Enamel indicated in black. B. Section of the same near the tip. Anterior side toward the right, exterior side upward. For orientation, compare Plate I, Fig. 2, and Plate III, Fig. 1.

Upper Teeth.— I^1 is very large, curved, with enamel on its anterior-superior and posterior-inferior surfaces (see Fig. 1). A cross section of this tooth is shown in Fig. 1B (note the postero-external flare). The enamel on the anterior-

superior surface extends into the alveolus to a position above P^1 , and the posterior inferior enamel ends above I^2 . The pulp cavity is almost closed at the root tip, indicating that the tooth did not grow persistently from an open root, as rodent incisors usually do. The tip is worn as indicated in Plate I, Fig. 2.

Although the crown of I^2 is missing from both sides of the holotype, the single root of each is preserved, and the tooth probably sloped in conformity with the posterior surface of the first incisor. Plate I, Fig. 2, and Plate III, Fig. 1, show the oval shape of the root of I^2 and its placement close to the posterior concavity of I^1 . The system of numbering the incisors 1 and 2 is used here without any intention of indicating the proper homology of these teeth, for it is not known.

I^2 is separated from the tooth behind it by a diastema of 1.5 mm. The premaxillary-maxillary suture zigzags across the middle of this space (Plate I, Fig. 2). Hence, the next tooth may be either the first premolar or the canine. The part of the tooth outside of the alveolus looks as if it had only one root, but excavation of the bone shows the presence of two roots. This fact, plus the unique crown shape of the tooth, indicates that it is probably a premolar, and it is here referred to that category. This P^2 has its roots strongly curved posteriorly above the end of I^1 . The crown is high and pointed, oval at its base, convex on the outside and concave on its inner surface. Its anterior face is uniquely grooved and has a thinner



TEXT FIG. 2. Horizontal mid-section of crown of right P^2 of *Sinclairiella*, $\times 10$. Anterior side toward the right, exterior side upward. For orientation compare Plate III, Fig. 1.

enamel coat than the rest of the tooth, as shown in Fig. 2, while the sides meet at the posterior edge to form a sharp blade. The anterior vertical trough or groove (See Plate

I, Fig. 2, and Plate III, Fig. 1), apparently accommodated the blade-like part of the third lower premolar to form an exceptionally efficient cutting device. A very short diastema lies between P^3 and the somewhat smaller P^4 .

P^4 also has two roots. A section at its crown base would have an irregular oval outline, wider at the rear than in front. A little tubercle appears at the base of the posterior cutting edge, and there is a small postero-internal shelf. One of the most interesting features of this tooth is its possession of a shallow vertical depression on its antero-internal border (Plate III, Fig. 1), corresponding to the groove which is such a prominent feature of P^3 . This trough of P^4 could not serve any such function in mastication as the homologous structure on P^3 , but seems to be another illustration of the frequently observed fact that when a tooth becomes greatly specialized for a particular purpose, its neighbors of the same series tend to be likewise modified, although the purpose of such similitude is unobvious.

M^1 can be seen in crown view in Plate III, Fig. 1 and in Plate I, Fig. 2. Like the other molars it is brachydont. The protocone is large and subconical; the subequal paracone and metacone have their anterior and posterior slopes modified by a continuous ridge which extends as a sharp cutting edge from the posterior edge of the small parastyle back to the posterior base of the metacone.

An external shelf or cingulum appears along the base of the metacone, narrow in front and widening backward. This cingulum supports a number of small cuspules, and although the largest of them are in the mesostyle and the metastyle positions, they are not true styles. Another short cingulum modifies the anterior base of the protocone. A deep fissure separates the conical hypocone from the protocone. There is a small but distinct paraconule at the interior base of the paracone, and a low rounded ridge at the postero-internal slope of the metacone. The triangular area pointed by the paracone, the metacone,

and the protocone is depressed into a deep hollow which, however, is exceeded in depth by another basin posterior to the protocone and between the metacone and the hypocone. This latter depression slopes downward to the posterior border of the tooth.

M² is wider from its external to its internal margins and shorter from front to back than M¹. The main cusps and basins of the two teeth are similar in placement and development, but M² has the parastyle antero-external to the paracone instead of anterior to it, and the external shelf is wide and elevated at its outer border into an irregular ridge. These features give the two teeth very different outlines. Crushing has displaced M²⁻³ with respect to the other cheek teeth (see the photograph, Plate I, Fig. 2), but the drawing, Plate III, Fig. 1, shows the position approximately corrected. M³ touches M² externally, but palateward the two teeth are divergent. See Plate I, Fig. 2. The trigonid of M₃ fits into this gap. M³ is simpler than M², with a smaller metacone than the latter. Although the enamel is worn in the localities of the hypocone and of the cingulum external to the metacone of M³, both structures were either absent or minute. The parastyle appears as a prominent cusp antero-external to the paracone, separated from the latter by a basin.

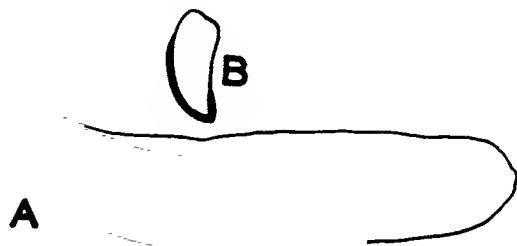
The upper molars of *Sinclairiella* are very similar to those of *Stehlinella* which are preserved, with, however, certain differences of structure and proportion, the most notable of which is the lack of an external cingulum on M¹⁻² in *Stehlinella*.

Lower Teeth:

The shape, size, and enamel distribution on the lower incisors can be seen in Fig. 3. The root of this tooth extends back to a rounded end below the middle of M₃ as shown in Fig. 4 (F). X-ray photographs of the left incisor, and high power microscopic examination of the root tip of the right one, indicate that the pulp cavity is not

widely open at the end of the root, as it is in rodent incisors, but rather, show that it narrows posteriorly

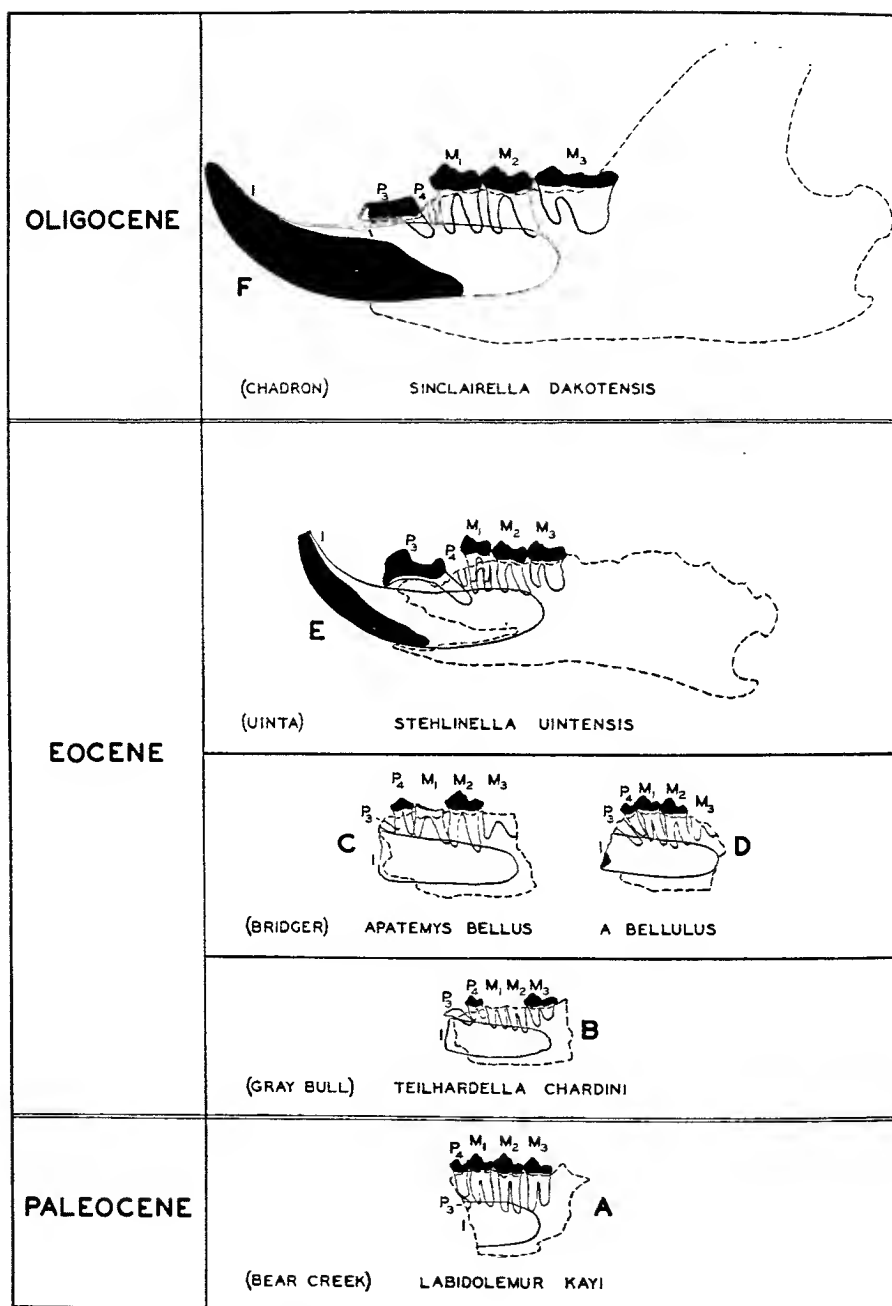
Although the anterior part of P_3 is broken off, it probably ascended to a thin elevated blade like that of *Stehlinella*. In P_3 of this latter genus the posterior crown margin rises to a large distinct cusp, whereas the corresponding part of *Sinclairiella's* P_3 has only a very slight



TEXT FIG. 3. A. Outline of left lower incisor of *Sinclairiella*, $\times 3$. Enamel indicated in black. B. Section of the same, external side toward the left.

elevation at the rear of the ridge which runs antero-posteriorly along the preserved part of the crown top. Skiagrams show that its single root slopes backward, external to the incisor root, at an angle of about 45° to a line along the bases of the molars (Fig. 4F). From P_3 to M_1 , the alveolar border rises sharply and is pierced midway between these two teeth by a small alveolus, indicated on Plate III, Fig. 3. P_4 had evidently fallen from this socket during the creature's life because the bone shows evidence of closing in, making it impossible to excavate the hole to its bottom. Examination by X-rays indicates that the alveolus widens slightly towards its base. The proclivous root of P_3 , however, so closely approaches the anterior root of M_1 , that P_4 had very little root space. This situation accords with the evolutionary decline of P_4 in the American APATEMYIDÆ, which will be discussed later.

M_1 has its protoconid and metaconid about equally developed. The paraconid is a very small tubercle of enamel which would not be recognized as a primary cusp except by tracing its progressive diminution from the



TEXT FIG. 4. Outlines of American Apatemyid lower jaws, drawn from skiagrams. $\times 2$ Enamel of teeth indicated in black; broken line is outline of specimen, dotted line is inference. *A*, *Labidolemur kayi*, Carnegie Mus. Cat. Vert. Foss., No. 11,703. *B*, *Teilharedella chardini*, Princeton No. 13236. *C*, *Apatemys bellus*, Am. Mus. No. 11425. *D*, *A. bellulus*, Am. Mus. No. 12048. *E*, *Stehlinella uintensis*, Am. Mus. No. 1903. *F*, *Sinclairella dakotensis*, Princeton No. 13585.

earlier Apatemyids. A ridge slopes downward and forward from the protoconid to the anterior edge of the tooth, where it expands to form a low cusp. The hypoconid appears as a distinct cusp upon the ridge which surrounds the deep-basined heel. Both the entoconid and the hypoconulid are mere bumps upon the lingual and the posterior parts, respectively, of the basin's rim. Outside of this rim, at the posterior base of the protoconid, the enamel is invaginated to form a deep groove. In consequence, there is a ridge down the postero-labial side of the protoconid. This ridge was worn by the animal's masticating habits, as was the external slope of that part of the heel rim which extends forward from the hypoconid to the base of the protoconid. Wear also planed the posterior slope of the metaconid.

M_2 has its paraconid developed as a very low cusp which continues the downward slope of the anterior surface of the metaconid. As on M_1 , the protoconid's anterior edge continues as a ridge to the front of the tooth. These features give the trigonid a quadrilateral aspect, and form an external cutting edge and an internal sloping shelf. The groove at the posterior base of the protoconid has been almost obliterated upon each second lower molar by a large cavity through the enamel and into the dentine (Plate III, Fig. 2, *C.A*). The dentine itself around each hole, as seen through the translucent enamel in a powerful light, shows evidence of decay. Since, however, the enamel is not greatly worn upon other parts of the molars, the inference that these cavities were caused or started by particular food habits seems justified. Further, the region of this cavity on each M_2 , was occluded with the paracone of the upper second molar. The paracone of the right M^2 is more battered and broken by wear than any other cusp of the upper molar series. (The left M^2 is not preserved.) The basined heel of M_2 is similar to that of M_1 .

M_3 has a sub-conical protoconid connected to the smaller metaconid by a ridge, similar to the condition on

M₂. The paraconid, however, is merely a low mound upon the antero-internal slope of the trigonid. The anterior flare at the base of the protoconid gives the trigonid the general aspect of a gently sloping plane which rises at its postero-external margin to the cone-like protoconid. A groove like that upon the two preceding molars is at the posterior base of the protoconid, external to the deeply basined heel. This heel has a large "third lobe" in the form of a low massive rounded cusp, the hypoconulid, whose antero-external face rises to a ridge that continues forward to the base of the protoconid, elevated midway by the sharp, ridgeform hypoconid. The internal rim of the heel's basin is much lower than the external ridge, and is somewhat irregular in height. The labial face of the protoconid is indented by a small dimple in the enamel. A corresponding feature is on M₁₋₂ in the form of a shallow depression, not produced by wear. The front edges of molars 2 and 3 overlap the posterior parts of molars 1 and 2, respectively, as shown in Plate II, Figs. 1 and 2.

The Skull:

Detailed description of many skull characters is inadvisable or impossible because of the crushed condition of the specimen. Further, the degree of specialization is so great that comparisons with most other mammalian skulls appear futile.

A top view, Plate I, Fig. 1, shows the two longitudinal ridges which run forward from the lambdoidal ridge and descend anteriorly into the zygomatic arch. These ridges rise and flare above the orbits, but there is no postorbital process. A small and inwardly inflected elevation on the zygomatic arch indicates the posterior boundary of the orbit, but there is no postorbital bar or plate such as that which completes the orbital ring of the Primates.

Although the brain case is large, the photographs, Plate I, Figs. 1 and 2, show it expanded laterally more than it was before crushing. Numerous foramina perforate it

upon the posterior parietal area (Plate I, Fig. 1), and upon the occipital region above the condyles. Plate I, Fig. 1, indicates the posterior expansion of the nasal bones, and the sutures between the parietals and frontals as far as they can be traced. The infraorbital foramen is large, as seen above M^1 in Plate I, Fig. 2, and the lacrymal foramen opens within the orbit. Details of the foramina of the auditory region can be seen in Plate I, Fig. 2. Displacement and crushing makes their accurate determination and description rather arbitrary. There is a partial tympanic ring, supported by a marginal wall involving the subjacent basicranial bones whose identity cannot be determined because the sutures are fused. In Plate I, Fig. 2, the condylar foramen can be seen immediately in front of the condyles, internal to the wide mastoid processes.

Great lateral and front-back movements of the lower jaw were permitted by the extraordinary flatness and large size of the glenoid fossa. It is nearly a plane surface.

The Lower Jaw:

The outside aspect of the jaw (Plates II, Figs. 1 and III, Fig. 3), shows the comparatively large and deep area for the insertion of the masseteric muscle. The anterior ridge of the ascending ramus arises opposite and below M_3 , so that the rear portion of the tooth is obscured in this view. Between this ridge and the back part of M_3 is a shallow depression or shelf, about the width of M_3 . The condyle articular surface is shaped like half an egg, and the jaw's angle appears slightly inflected. A groove behind the incisor alveolus deepens to the posterior mental foramen below the back half of M_2 . A smaller hole directs forward from this main foramen under the anterior edge of M_2 . Below M_1 the jaw bone is depressed as a low hollow, remarkable chiefly for its evolutionary derivation from earlier Apatemyids which have it more strikingly developed.

An internal view of the jaw shows the progressive flattening of the paraconid from M_1 - M_3 (Plate II, Fig. 2). Note

also the presence of a small foramen near the lower jaw border, below the anterior part of M_1 , on the symphyseal region. The symphyseal scar is wide and rather smooth, with no clearly marked boundaries.

MEASUREMENTS (IN MILLIMETERS)

Overall length of skull	57.0
$I-M_1$ (approximate alveolar distance)	25.8
I^1 ant.-post. diam. at alveolus	6.1
I^1 transverse diam. at alveolus	2.8
M_1^{1-2}	10.9
Overall length of jaw	45.0
Depth below M_2	9.7
I length from tip to end of root	23.8
Vertical diam. I at alveolus	4.9
Transverse diam. I at alveolus	2.4
Alveolar rim to tip of I	11.0
$I-M_3$	27.0
M_2-M_3	12.4
M_1 Ant.-post. diameter	3.5
Transverse diameter	2.4
M_2 Ant.-post. diameter	3.7
Transverse diameter	2.8
M_3 Ant.-post. diameter	5.5
Transverse diameter	2.6

APATEMYIDÆ AND PLESIADAPIDÆ

As here redefined, the APATEMYIDÆ can be distinguished from the Plesiadapid genera *Plesiolestes*, *Pronothodectes*, and *Plesiadapis*, by these characters:

APATEMYIDÆ	PLESIADAPIDS
1. Dental formula $\frac{2-0-2-3}{1-0-2-3}$	$\frac{2-1-3-3}{1-0-4}$ (or $\frac{2-0-4-3}{2-3}$)
2. Root of lower incisor ends under M_3	Ends anterior to M_2
3. Blade-like P_3	"Normal" P_3
4. P_3 single rooted	P_3 two rooted
5. Depression on jaw under P_4-M_1	No depression
6. Posterior mental foramen under M_1	Under P_4 or more anterior ¹
7. Infraorbital foramen above M^1	Above P^3
8. No apical cusps on anterior upper incisor	Two or three apical cusps
9. No enamel on sides of anterior upper incisor	Enamel covered crown
10. P^{2-1} two rooted, single cusped	Three rooted, three cusped
11. No mesostyles on M^{1-2}	Prominent mesostyles on M^{1-3}
12. American range from U. Paleocene (Bear Creek) to L. Oligocene (Chadron)	American range from L. Paleocene (Torrejon) to L. Eocene (Gray Bull)
13. European range restricted to Eocene (U. Landenian to Bartonian)	European range from U. Paleocene (Thanetian) to L. Eocene (Sparnacian)

¹Matthew states that the posterior mental foramen of *Plesiadapis gidleyi* is under M_1 , but a recheck of all available specimens fails to show it in this position on any of them.

Further information may compel modification of several of these diagnostic differences between the two families. The American species of Apatemyids are *Labidolemur kayi*, *L. soricoides*, *Teilhardella chardini*, *Apatemys bellus*, *A. bellulus*, *A. rodens*, *Stehlinella uintensis*, and *Sinclairiella dakotensis*. European species which possibly may be included in the family are *Eochiromys landenensis*, *Heterohyus quercyi*, *H. armatus*, *H. europæus*, *H. heufelderi*, and *H. nanus*. It is possible to split this grouping into two, as suggested by Teilhard, on the basis of the enamel distribution on the lower incisor. *Eochiromys* is stated to have its incisor crown entirely enamel covered. *Labidolemur soricoides* incisors are similarly coated. In addition, the enamel does not extend into the alveolus, as it does in other Apatemyids.

There is another possibility. Perhaps two distinct types of jaws and incisors are confused in *Eochiromys* and *Labidolemur*. Each genus may include two forms, but more material is necessary before definite statements become possible.

Labidolemur kayi (See Fig. 4A) from the Bear Creek of Montana has been restudied (after the closely adhering matrix of pyrite and coal tar products was removed by newly developed methods), and now some of the suggestions expressed by Simpson can be emphasized with greater certainty or corrected. This species differs in several important structures from the type of the genus, *Labidolemur soricoides*, of the Colorado Tiffany. The specimens of this latter species, however, are too fragmentary and crushed to state with confidence that *Labidolemur kayi* and *L. soricoides* are different genera. Molar teeth of many small Paleocene mammals resemble each other, although other structures place the individuals in widely separated orders.

Figure 4B shows the Gray Bull Apatemyid, *Teilhardella chardini*. It is smaller than the other Apatemyids and P₄ has two roots. Bridger forms of *Apatemys* show considerable puzzling variation. The extreme types which Matthew described as *A. bellus* and *A. bellulus* are figured (Figs. 4C and 4D). *A. bellus* is larger, has a large two-rooted P₄ and widely

divergent roots of M_1 , in contrast to the small single-rooted P_4 and the closely crowded roots of M_1 of *A. bellulus*. However, other specimens of *Apatemys* bridge these structural gaps, and most surface features of the two species are very similar. X-ray studies of other groups of fossil mammals will probably show that internal structures are most revealing in attempting classifications.

Stehlinella uintensis is pictured in Fig. 4E. Matthew unfortunately did not see the alveolus of P_4 . Its observation would have explained the dilemma in which he found himself when he tried to derive P_3 of *Stehlinella* (which he called P_4) from P_4 of *Apatemys*.¹ The conjectural skull reconstruction of *Stehlinella* in the type description can now be corrected by comparison with *Sinclairiella*. The great flaring nasal openings, as shown in Figs. 1 and 2 of Matthew's paper, are actually the alveoli of the anterior incisors, and the diastema there indicated between the anterior tooth (I^2 and not I^1) and P^3 should be reduced to about half the distance upon the reconstruction. The anterior root of M_1 had been placed on the alveolus of P_4 . Figure 4E shows also that the roots of M_1 are part way out of the alveoli.

P_4 had dropped out of the jaw of *Sinclairiella* also (see Fig. 4F), and the alveolar bone had begun to close in around the alveolus before the creature's death.

By comparing the outline drawings of Fig. 4 certain of the evolutionary developments of the American *Apatemyidae* can be traced from *Labidolemur kayi* to *Sinclairiella dakotensis*. Most obvious of these trends is the diminution and crowding of P_4 . Although this tooth is small and "degenerate" in *Labidolemur*, its crown tip is not much below the level of the protoconids of the molars. Its length (anterior-posterior) is less in proportion to the length of the molar row in the succeeding *Apatemyids*, and its crown becomes lower with respect to the height of the molars.

¹W. D. Matthew, "Stehlinus, A New Eocene Insectivore." *Am. Mus. Novitates*, No. 14, Sept. 7, 1921, p. 1. See also P. Teilhard, "Les Mammifères de l'Eocene Inferieur Francais et leurs Gisements," *Annales de Paleontologie*, 1921, p. 94.

The heel of M_3 is seen to lengthen progressively in comparison with the trigonid of this tooth. Certain of these relationships are not clearly defined upon the diagrams, and can be deduced from the specimens only by measurements, due to the deceptive appearance which structural configurations, color variations, and size differences lend to the types. The paracoid becomes smaller. Indeed, as noted above, it would not be called a cusp on *Sinclairella* teeth except by observation of its gradual decline in the earlier genera.

These observations are not intended to favor a suggestion that the Apatemyid series represents a straight ancestor-descendent sequence. There are too many minute details of structural differences and too much variation within the genus *Apatemys* to justify such a guess, but close relationship (of no one knows how many sub-species, races, or varieties) of the component genera and species is favored by most of the observed skull, jaw, and dental characters.

The American PLESIADAPIDÆ as here limited include *Pronothodectes matthevi*, *Plesiolestes problematicus*, *Plesiadapis gidleyi*, *P. fodinatus*, *P. dubius*, and *P. cookei*; and the European species are *P. tricuspidens*, *P. orsmaelensis*, *P. richardsoni*, *P. daubrei*, and *Chiromyoides campanicus*.

These lists omit *Ignacius*, *Phenacolemur*, *Trogolemur*, and *Uintasorex*. *Ignacius*, as noted above, probably is congeneric with *Phenacolemur*. *Phenacolemur* itself presents a cluster of what may be regarded as family characters which exclude it from close relationship with either the Apatemyids or the Plesiadapids as defined above. This paper, however, does not attempt to discuss the proper position of the genus. *Uintasorex* and *Trogolemur* were included in the APATEMYIDÆ (and, later, in the PLESIADAPIDÆ) by their nomenclator, Matthew. Neither genus has any claim to either family. Schlosser referred *Trogolemur* to the ANAPTOMORPHIDÆ.¹ Matthew repeatedly gave the lower dental formula of *Trogolemur* as 1-0-3-3, although an additional tooth root shows below P_2 on the published photograph of the type, Plate LII,

¹ Karl A. von Zittel, *Grundzüge der Paläontologie* (Schlosser Edition of 1911), p. 549.

Fig. 5.¹ Surface and X-ray examination of the specimen clearly show this additional root, making the formula 1-0-4-3, assuming that the canine has disappeared. Tooth homologies are difficult to ascertain in these groups of mammals of uncertain ordinal position. Until definitive material is obtained, however, there seems to be little cause for concern whether the tooth immediately behind the enlarged front tooth is called a canine or a premolar, as long as the determinations are consistent. Matthew called the second tooth of *Uintasorex* (represented only by its alveolus) a canine, but labeled this second tooth as the first of the premolar series in the other "Plesiadapids." For uniformity's sake, the lower tooth formula of *Uintasorex* might better be written 1-0-3-3 instead of 1-1-2-3.

The posterior mental foramen of *Uintasorex* is beneath P₄. The second questionable position which Matthew gave as ? M₂ is a dent in the bone. The incisor of *Uintasorex* ends below M₁.

SUMMARY

The family APATEMYIDÆ Matthew 1909, as herein redefined, consists of a group of genera, apparently closely related to each other, whose ordinal placement is at present highly arbitrary.

Labidolemur kayi from the upper Paleocene is the earliest known member of the family. The skull and jaws of a new genus and species, *Sinclairiella dakotensis*, from the Chadron formation extends the family's geologic range into the lower Oligocene of America. *Labidolemur kayi* shows such advanced specializations as to obscure the true ordinal relationships of these unique mammals. It may be necessary, therefore, to seek among earlier faunas for clues to the connection of the APATEMYIDÆ with related stocks.

Evolutionary changes can be traced from the Bear Creek species *Labidolemur kayi*, through the Gray Bull lower Eocene *Teilhardella*, the Bridger middle Eocene *Apatemys*, the Uinta upper Eocene *Stehlinella*, to the lower Oligocene *Sinclairiella*.

¹ W. D. Matthew, "The Carnivora and Insectivora of the Bridger Basin," *Memoirs of the Am. Mus. Nat. Hist.*, Vol. IX, Part VI, 1909.

Paleocene and lower and middle Eocene Apatemyid specimens consist of broken lower jaws. The fragmentary skull of *Stehlinella* and that of *Sinclairiella*, have few characters to justify placing the family in any known order of mammals.

Those structures which indicate Primate relationships are contradicted by others, equally convincing, to place the family elsewhere. The (at present) terminal member of the line, *Sinclairiella*, has no structures to link it with *Chiromys*. Matthew's plan of putting the APATEMYIDÆ among the INSECTIVORA is followed for the sake of convenience in keeping the Primates more compact, and because the fossil INSECTIVORA are already a reservoir of groups with undefinitive characters. This artifice is subject to later revision when and if more skeletal material is found. Later work may dictate the advisability of creating a separate order to receive the APATEMYIDÆ. The family furnishes another illustration of very early and great specialization and persistent geologic range.

Characters of the skull, jaws, and teeth of the APATEMYIDÆ clearly separate this family from the residue of the PLESIADAPIDÆ and emphasize the previously made suggestion that this latter family has heretofore included several phyletic lines of curiously specialized mammals whose resemblance may be due to convergent adaptations and not to close relationship. Additional work with skiagrams of known specimens, and the discovery of additional ones, may permit the remaining "Plesiadapids" to be further separated into groups whose true affinities can be satisfactorily ascertained.

PLATE I



FIG. 1. *Sinclairella duboensis*. Holotype, Princeton No. 13585. Top view of skull. Retouched photograph, $\times 2$.



FIG. 2. Palatal view of the same, $\times 2$.

PLATE II

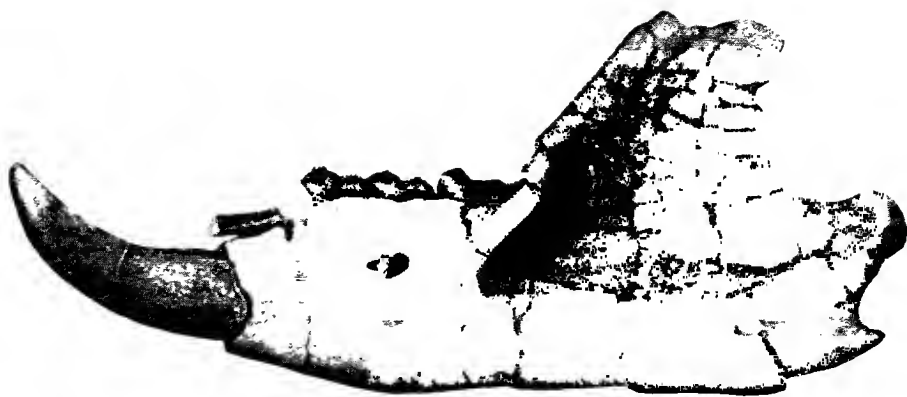


FIG. 1. *Sorlairella dabovi*, *n*. Holotype, Princeton No. 13585. Outside view of left lower jaw. Retouched photograph, $\times 2\frac{1}{2}$.



FIG. 2. Inside view of the same, $\times 2\frac{1}{2}$.

PLATE III

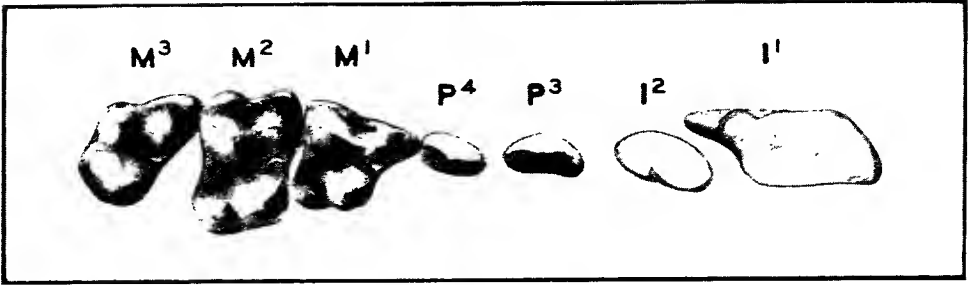


FIG. 1. Crown view of right upper teeth of *Simulium labreana*. Princeton No. 135851. $\times 4$.

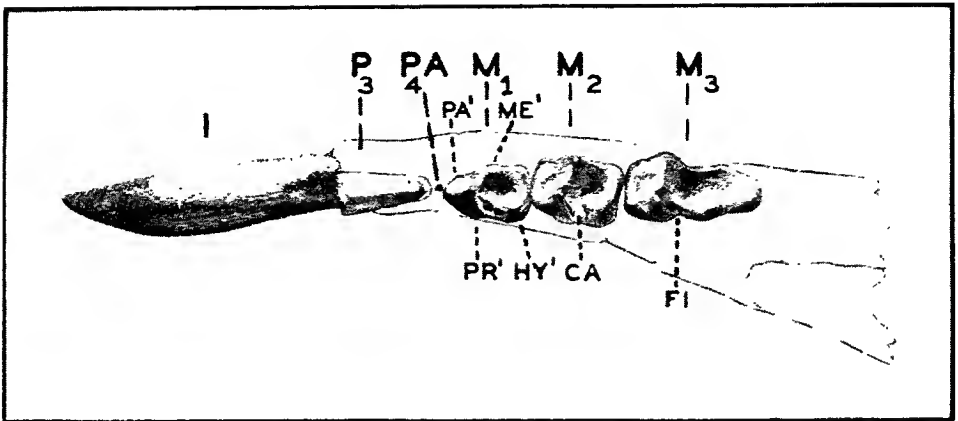


FIG. 2. Crown view of left lower teeth of *Simulium labreana*. $\times 3.4$. *PA'* paraconid, *ME'* metaconid, *HY'* hypoconid, *CA* cavity, *FI* fissure.

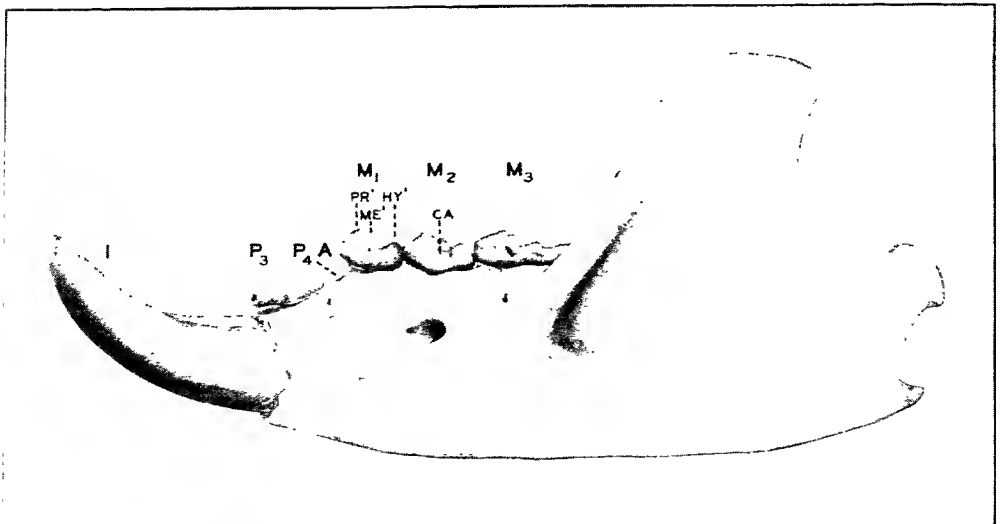


FIG. 3. Outside view of left lower jaw of same, $\times 2\frac{1}{2}$. Symbols as in Fig. 2 above.

THE PETROGRAPHY OF SOME ROCKS FROM SOUTH VICTORIA LAND *

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GENERAL STATEMENTS

DR. LAURENCE M. GOULD,¹ geologist of the Byrd Antarctic Expedition, 1928-30, collected 21 igneous and 19 metamorphic rocks in the Mount Fridtjof Nansen district of the Queen Maud Mountains, Antarctica. These fault-block mountains form the southern boundary of the Ross Shelf Ice and, in South Victoria Land, are made up of a thick series of sedimentary rocks, intruded by diabase sills, known as the Beacon Sandstone² which rest upon a pre-Cambrian basement complex. The only other rocks collected in this area were those gathered, in 1911, at Mount Betty by Roald Amundsen on his South Pole Expedition, and according to Schetelig,³ the suite, mainly granites and gneisses, numbers some 20 pieces.

PETROGRAPHY OF THE IGNEOUS ROCKS

Gould collected 21 igneous rocks in the Mount Fridtjof Nansen district of the Queen Maud Mountains, and the types are: Leucogranite, pegmatite, leucogranitic aplite, granodiorite, porphyritic granodiorite, tonalite, gabbro, melabasalt, and diabase. Sixteen small specimens of prehnite, epidote, quartz, and feldspar were collected in the upper portion of an intrusion of melabasalt in the western slope of Mount Fridtjof

* Part of a dissertation submitted in partial fulfillment for the requirements for the degree of Doctor of Philosophy, in the University of Michigan.

¹ Gould, L. M., "Some Geographical Results of the Byrd Antarctic Expedition," *Geogr. Rev.*, Vol. XXI, No. 2, pp. 177-200, April, 1931. "Structural Relationships of the Queen Maud Mountains, Antarctica," *Proc. Geol. Soc. America* (Abs.), 1934.

² Stewart, Duncan, "The Petrography of the Beacon Sandstone of South Victoria Land," *Am. Mineralogist* (in press), August, 1934.

³ Schetelig, J., "Report on the Rock Specimens Collected on Roald Amundsen's South Pole Expedition," *Videnskapsselskabet's Skrifter, I, Mat.-naturv. Klasse*, No. 4, pp. 1-31, Christiania, 1915.

TABLE I
MINERALOGICAL COMPOSITION OF SOME IGNEOUS ROCKS FROM THE QUEEN MAUD MOUNTAINS OF SOUTH VICTORIA LAND

Mineral	Specimen											
	2	2a	30	19	15	29b**	22b	7 ⁴³	16 (1)	16 (2)	16 (3)	21
Quartz	30.60	p	35.61	23.67	30.64	41.14	2.06	1.43	p	p	p	.
Orthoclase	{ 45.40	p	36.40	9.22	15.48	.	0.81	7.49
Microcline		p	
Microperthite	.	p
Albite	.	p
Olivoclase	16.50	.	25.09	58.00	44.97	41.74
Labradorite	41.14	50.01	72.66	75.66	52.55	66.47
Bytownite
Muscovite	2.60	p	1.13	.	.	1.10
Garnet	.	.	p
Enstatite
Augite
Biotite	4.80	p	{ 1.57*	{ 9.11	abs	15.15	{ 55.49	{ abs	{ 27.03	{ 23.99	{ 47.17	{ 32.82
Chlorite	p	.				.						
Hornblende	.	.	p
Epidote	.	p	p	.	8.90	0.42	0.49	0.70	0.32	0.35	0.28	0.72
Iron "ores"	.	p	p	.	.	p
Calcite	.	p	p	p	.	.	p	p
Titanite	p
Rutile	?	p
Apatite	{ 0.10	.	p	p	p	{ 0.13	p	p	p	p	p	p
Zircon		p	p	p
Tourmaline	.	p
Topaz
Leucocene	.	p	.	p	.	.	p	p	p	p	p	p
	100.00	.	99.80	100.00	99.99	97.98	99.99	99.99	100.01	100.00	100.00	100.01

p = present in thin section.

* Includes all mafics.

** Average of two thin sections.

29b. Tonalite. Mount Betty.

22b. Melabasalt. Western slope of Mount Fridtjof Nansen.

7. Diabase. Eastern slope of Mount Fridtjof Nansen.

16. Gabbro. Western slope of Mount Fridtjof Nansen.

21. Gabbro. Same locality.

2. Leucogranite. Midway between Stations 1 and 2.

2a. Pegmatite. Same locality.

30. Leucogranitic aplite. Mount Betty.

19. Granodiorite. Western slope of Mount Fridtjof Nansen.

15. Porphyritic granodiorite. Same locality.

TABLE 2

MINERALOGICAL COMPOSITION OF SOME METAMORPHIC ROCKS FROM THE QUEEN MAUD MOUNTAINS OF SOUTH VICTORIA LAND

Mineral	Specimen														
	4*	4**	32a	1*	1**	28*	28**	29*	29a**	25*	25**	24a	Y	Z	X
Quartz.....	19.56	35.73	47.74	33.33	34.60	11.45	27.17	55.84	62.46	30.49	30.36	...	13.88	20.50	53.84
Orthoclase.....	0.12	1.89	p
Plagioclase.....	0.72	...	4.84	40.14	33.52	23.99	24.22	58.38	62.93	...	38.35	p	p
Biotite.....	67.47	51.80	{36.48	25.23	21.83	48.14	38.89	{16.32	{10.21	8.68	3.55	17.95	25.83
Chlorite.....	p	p
Muscovite.....	12.07	11.56	9.60	41.09	43.49	2.53	2.40	0.19	0.20	12.97	...
Hornblende.....	74.72	21.94	62.56	45.80
Diopside.....	1.15
Tourmaline.....	0.05	{0.22	{0.08	{0.27	{0.42	0.13	p	...	p
Zircon.....	0.18	...	0.06	1.31	0.70	p	0.10	...
Apatite.....	2.05	p
Epidote.....	...	0.93	{0.26
Titanite.....	abs	abs	{7.20	p	...	p
Iron "ores".....	0.35	p	...	p
Leucoxene.....
Sillimanite.....	?	?	p	?	?	p	p
Rutile.....
Zoisite.....
Garnet.....
Calcite.....	p
Dolomite.....	p
Brucite.....	p
Talc.....
Chalcopyrite.....	p
Malachite.....	p
	100.00	100.02	100.27	99.99	100.00	100.00	100.00	99.99	99.99	100.00	100.00	100.00	100.00	99.97	100.00

p = present in thin section.

* Section cut parallel to the schistosity.

** Section cut perpendicular to the schistosity.

4. Biotite schist. Midway between Stations 1 and 2.

32a. Biotite schist. Mount Betty.

1. Muscovite-biotite schist. Base of cliff, 2 miles east of foot of Liv Glacier.

Y. Biotite-hornblende gneiss. Midway between Stations 1 and 2.

Z. Contact quartzite. Same locality.

X. Contact quartzite. Same locality.

24b. Contact rock exhibiting copper signs. O'Brien Peak.

24c. Contact rock exhibiting copper signs. Same locality.

26. Impure gray marble. O'Brien Peak.

26a. White marble. Same locality.

28. Biotite-oligoclase schist. Mount Betty.

29. Biotite gneiss. Mount Betty.

25. Oligoclase-biotite gneiss. O'Brien Peak.

24a. Hornblende schist. O'Brien Peak.

Nansen. Six of the rocks have been analysed chemically.⁴ Table 1 gives the mineralogical compositions of the igneous rocks. The quantitative data was obtained with the improved Wentworth recording micrometer.⁵

PETROGRAPHY OF THE METAMORPHIC ROCKS

Nineteen specimens of metamorphic rocks were collected by Gould in the Queen Maud Mountains in the vicinity of Mount Fridtjof Nansen, and the types represented are: Biotite, muscovite-biotite, biotite-oligoclase, and hornblende schists, biotite, oligoclase-biotite, biotite-hornblende, and hornblende gneisses, contact quartzite, contact rock exhibiting copper signs, impure gray marble, and white marble. Chemical analyses of two of these specimens have been recorded.⁶ Table 2 gives the mineralogical compositions of the metamorphic rocks.

ACKNOWLEDGMENTS

The author wishes to thank Dr. Laurence M. Gould for the privilege of examining his collection of specimens, and to acknowledge the active interest and advice of Professor William H. Hobbs and Professor Walter F. Hunt.

⁴ Stewart, Duncan, "The Petrography of Some Antarctic Rocks," *Am. Mineralogist*, Vol. 19, No. 4, pp. 150-160, April, 1934.

⁵ Hunt, W. F., "An Improved Wentworth Recording Micrometer," *Am. Mineralogist*, Vol. 9, No. 9, pp. 190-193, 1924.

⁶ Stewart, *Op. cit.*, p. 154.

THE UNIVERSITY OF MICHIGAN COLLECTIONS OF ANTARCTIC ROCKS AND MINERALS

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INTRODUCTION

PROFESSOR WILLIAM H. HOBBS, Head of the Department of Geology in the University of Michigan, has been actively interested during the past few years in assembling at the University of Michigan a series of Antarctic rocks and minerals. The extensive rock collections made by Professor Laurence M. Gould, second-in-command of the Byrd Antarctic Expedition, 1928-30, were, in the late Spring of 1931, entrusted to Professor Walter F. Hunt, now Head of the Mineralogical Laboratory in the University of Michigan, who kindly permitted the author to analyse the valuable specimens petrographically. Professor Hobbs immediately realised the advantages to be derived from a study of additional material for comparative purposes, and wrote to Dr. W. Campbell Smith, Sir Douglas Mawson, Dr. Erich von Drygalski, Professor A. Lacroix, Dr. Gregori Aminoff, and Dr. R. N. Rudmose Brown for duplicate specimens collected by seven other Antarctic expeditions. Due to the splendid cooperation of these men the University of Michigan, through exchange material, has obtained this remarkable set now comprised of a total of 294 specimens of Antarctic rocks and minerals. By the Spring of 1934, the specimens of six of the eight Antarctic expeditions had been studied petrographically, and this research constituted the author's doctoral dissertation in 1933. Summary petrographical articles are being published in scientific periodicals, and it is hoped that it will be possible to continue these studies in the near future the results of which will be monographed.

THE EIGHT COLLECTIONS OF ANTARCTIC
ROCKS AND MINERALS

Dr. Laurence M. Gould collected 103 rock and mineral specimens in the Queen Maud Mountains of South Victoria and Marie Byrd Lands, and in the Rockefeller Mountains, King Edward VII Land (Fig. 1). The following are the types:

A. South Victoria Land:*a.* Igneous (21 specimens):

Leucogranite, pegmatite, leucogranitic aplite, granodiorite, porphyritic granodiorite, tonalite, gabbro, melabasalt, and diabase.

b. Sedimentary (Beacon Sandstone formation) (27 specimens):

Arkose, shaly arkose, micaceous arkose, contact arkose, "spotted" arkose, arkose with quartzitic arrangement of quartz, ferruginous micaceous arkose, and slaty graywacke.

c. Metamorphic (19 specimens):

Biotite schist, muscovite-biotite schist, biotite-oligoclase schist, hornblende schist, biotite gneiss, oligoclase-biotite gneiss, biotite-hornblende gneiss, hornblende gneiss, contact quartzite (?), contact rock exhibiting copper signs, impure gray marble, and white marble.

d. Miscellaneous:

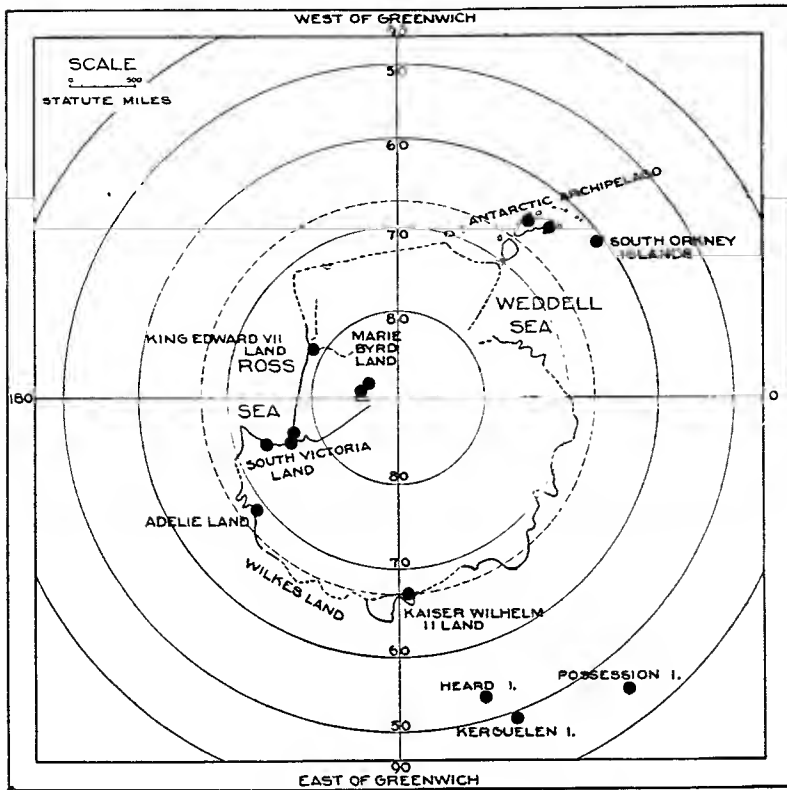
Four hundred rock and mineral pebbles from the stomachs of Emperor penguins; sixteen specimens of vein and cavity fillings of epidote, feldspar, quartz, and prehnite; one incrustation of calcite and gypsum.

B. Marie Byrd Land:*a.* Metamorphic (seven specimens):

Hornblende-biotite schist, orthoclase-biotite schist, biotite-orthoclase schist, and biotite schist.

b. Mineral (one specimen):

Prehnite.



Sketch map of Antarctica showing the distribution of rock specimens in the University of Michigan collections.

C. King Edward VII Land:

a. Igneous (eight specimens):

Monzogranite, alaskite, acid dike rock, porphyritic leucomonzogranite, beryl-bearing pegmatite, leucogranite, and leucogranitic aplite.

b. Minerals (three specimens):

Crystalline and cryptocrystalline quartz.

Dr. W. Campbell Smith, Curator of the Department of Geology, British Museum of Natural History, contributed 18 duplicate specimens collected *in situ* from the Beacon Sandstone formation by the National Antarctic Expedition, 1901-04, in the Ferrar Glacier district, South Victoria Land, as well

as, 12 duplicate rock specimens, mainly Beacon Sandstone erratics, which were obtained in the majority of cases from the Priestley Glacier Moraine, Terra Nova Bay district, collected by the British Antarctic (*Terra Nova*) Expedition, 1910-13. The following are the rock types:

A. South Victoria Land (Ferrar Glacier district):

a. Sedimentary (15 specimens):

Conglomerate, fine-grained conglomerate, coarse sandstone, sandstone, limonitic sandstone, ferruginous sandstone, arkose, quartzitic arkose pebble, and siltstone.

b. Igneous (three specimens):

Highly altered extrusive rocks (?).

B. South Victoria Land (Terra Nova Bay district):

a. Sedimentary (10 specimens):

Coarse sandstone and impure coarse sandstone containing "charred wood" remains, yellow sandstone, black sandstone, contact sandstone, and shale.

b. Igneous (two specimens):

Altered rhyolite porphyry.

Mr. R. E. Priestley of Cambridge University contributed through Professor Hobbs a specimen of kelyte and a large anorthoclase crystal from Ross Island, South Victoria Land.

Sir Douglas Mawson, Leader of the Australasian Antarctic Expedition, 1911-14, donated 12 duplicate specimens of metamorphic rocks collected in Adelie Land: Granodiorite gneiss, biotite-hornblende-feldspar gneiss (erratic), granite gneiss, plagioclase-pyroxene gneiss, hypersthene-alkali feldspar gneiss, garnet-cordierite gneiss, hypersthene-biotite-feldspar gneiss, magnetite-garnet schist (erratic), biotite-hornblende schist (erratic), amphibolite, forsterite-marble (erratic), and epidote-marble (erratic).

Dr. Erich von Drygalski, Leader of the Deutsche Südpolar Expedition, 1901-03, furnished 15 duplicate basaltic rocks collected by his Expedition in the African Quadrant of the Antarctic. The types noted are:

- A. Kaiser Wilhelm II Land (Gaussberg):
 - a. Igneous (six specimens):
Leucite basalt, augite-leucite nodule, and an inclusion in basalt.
- B. Kerguelen Island:
 - a. Igneous (five specimens):
Plagioclase basalt.
- C. Heard Island:
 - a. Igneous (two specimens):
Plagioclase basalt and magma basalt.
- D. Possession Island:
 - a. Igneous (two specimens):
Plagioclase basalt.

Professor A. Lacroix, of the *Muséum National d'Histoire Naturelle* in Paris, gave 16 duplicate rock specimens collected in the islands of the western area of the Antarctic Archipelago by the *Expédition Antarctique Française, 1903-05*. The types noted are:

- a. Igneous (15 specimens):
Granodiorite,¹ tonalite,¹ melagabbro,¹ aplite,¹ soda-pyroxene-amphibole microgranite,² quartz diabase,¹ andesite,¹ hornblende andesite,² oligoclase andesite,³ andesite porphyry,¹ and dacite porphyry.¹
- b. Metamorphic (one specimen):
Quartz schist.¹

Dr. Gregori Aminoff, of the Mineralogical Department, Riksmuseet, Sweden, contributed 107 duplicate rock specimens collected in the northeastern area of the Antarctic Archipelago by the Swedish Antarctic Expedition, 1901-03. The specimens were obtained from Cockburn, Snow-Hill, Seymour and James Ross Islands, Crown Prince Gustav Canal, and Hope Bay, Louis Philippe Land. The pieces are mainly sedimentary and igneous types, although a few metamorphic rocks are noted.

¹ Name given by Stewart.

² Name given by Gourdon.

³ Name given by Lacroix.

Dr. R. N. Rudmose Brown, of the University, Sheffield, England, has added materially to the series by supplying nine duplicate sedimentary rocks obtained on Laurie, Saddle, Coronation, and Graptolite Islands, South Orkney Islands, by the Scottish Antarctic Expedition, 1902-04. The rocks are: Graywacke, conglomerate, and sheared and cleaved shale.

PETROGRAPHICAL STUDIES OF THE ROCKS

The specimens collected by Dr. Laurence M. Gould have all been examined petrographically, and 15 chemical analyses have been added to the previously recorded 158 analyses of rocks from the Antarctic Continent and Antarctic Archipelago. The duplicate specimens collected by the National Antarctic Expedition, with the exception of preliminary descriptions of five of the rocks, had not been previously thoroughly studied with the microscope, and petrographical data on the duplicates obtained by the British Antarctic (*Terra Nova*) Expedition was lacking. The material obtained by the Australasian Antarctic Expedition had all been beautifully monographed, as had the rocks collected by the Deutsche Südpolar Expedition, 1901-03. The occurrence, qualitative petrographical results, and chemical analyses of the rocks collected by the Expédition Antarctique Française, 1903-05, had been recorded in the scientific reports of that Expedition. The material gathered by the Swedish Antarctic Expedition had been in part monographed in Expedition reports, whereas, complete petrographical data was lacking for the rocks collected by the Scottish Antarctic Expedition, 1902-04, in the South Orkney Islands.

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INDEX TO VOLUME LXXIV

- Action of anæsthetics on living protoplasm, L. V. Heilbrunn, 159
- Administrative problems of borough organizations, Bradford W. West, xvii
- Æsthetic and sociological aspects of city and regional planning, Jacques Grèber, 15
- Agassiz, Louis, xxiii
- Albright, W. F., The chalcolithic or copper-stone age in the Near East, xv
- American Apatemyidæ and the description of a new genus, *Sinclairiella*, from the White River Oligocene of South Dakota, A revision of the (Jepsen), 287
- American Bacteriologists Society, vii
- Anæsthetics on living protoplasm, The action of (Heilbrunn), 159
- Apatemyidæ and the description of a new genus, *Sinclairiella*, from the White River Oligocene of South Dakota, A revision of the American (Jepsen), 287
- Asia, New light on prehistoric man in (MacCurdy), 185
- Atlas of New England, The linguistic (Kurath), 227
- Beatty papyrus of the Epistles of Paul, Henry A. Sanders, xv
- Benedict, Francis G., A simple, rapid basal metabolism measurement for hospital routine, xi
- Blakeslee, A. F., A new synthesized pure-breeding chromosome type in the Jimson weed, *Datura stramonium*, xv
- Brown, Ernest W., Time and its determination, viii
- Bryant, W. L., New fishes from the Triassic of Pennsylvania, xi
- Cannon, Annie J., A spectral survey of the nearest galaxy, the large magellanic cloud, xi
- Carter, Edward W., Reorganization of the second class township, xvii
- Chalcolithic or copper-stone age in the Near East, W. F. Albright, xv
- Chromosome structure in *Impatiens*, Prochromosomes and (Smith), 193
- City and regional planning, Æsthetic and sociological aspects of (Grèber), 15
- Cleland, Ralph E., Cyto-taxonomic studies on certain cenothers from California, x
- Committee on Grants, xii, xvii
- Committee on Nominations, viii
- Committee on Use of Funds, *vide* Committee on Grants, xii, xvii
- Compton, Karl T., xii
- Conklin, Edwin G., R. A. F. Penrose, Jr., Memorial Lecture: A generation's progress in the study of evolution, 137
- Committee on Grants—Report, xii, xvii
- Conservation and evolution in a changing social program, John C. Merriam, viii
- Cordyceps* and *Ophiocordyceps* in Michigan, The genera (Mains), 263
- Council, Proceedings submitted, xii
- Crile, George, Solar radiation, lightening, the azotobacter and protoplasm, xi
- Currency, Stabilization of the (Westerfield), 29
- Cyto-taxonomic studies on certain cenothers from California, Ralph E. Cleland, x
- Davis, Bradley Moore, What is *Ænothera Hookeri* Torrey and Gray, 245
- Delano, Frederic A., The economic implications of national planning, 21
- Dixon, Roland B., The long voyages of the Polynesians, 167
- Duane, Russell, xxiii
- Duggar, B. M., Some physico-chemical properties of the virus of typical tobacco mosaic, x
- Duplicate tax assessments, Blake E. Nicholson, xvii
- Economic functions of the stock market as a business agency and the proposed National Securities Exchange Act of 1934, S. S. Huebner, 77
- Economic implications of national planning, Frederic A. Delano, 21

- Evolution, A generation's progress in the study of, The R. A. F. Penrose, Jr., Memorial Lecture (Conklin), 137
- Excavations of the University Museum at Minturnæ, Jotham Johnson, xxiii
- Factors controlling prices, domestic and international, Ernest Minor Patterson, 63
- Fish melanophores, Neurohumors as activating agents for (Parker), 177
- French Government, xvii
- Genera *Cordyceps* and *Ophiocordyceps* in Michigan, Edwin B. Mains, 263
- Generation's progress in the study of evolution, R. A. F. Penrose, Jr., Memorial Lecture, (Conklin), 137
- Genetic constitution in relation to pathology, Some aspects of (Gowen), 215
- Geomorphic investigations in the Yellowstone Park and Big Horn regions of Wyoming, Douglas Johnson, xi
- Government ownership and operation of the railroads, Problems of (Johnson), 103
- Governmental aspects of regional planning, Some (Radcliffe), 1
- Gowen, John W., Some aspects of genetic constitution in relation to pathology, 215
- Gray, What is *Oenothera Hookeri* Torrey and (Davis), 245
- Grèber, Jacques, Esthetic and sociological aspects of city and regional planning, 15
- Gregory, William K., A half-century of trituberculy, the Cope-Osborn theory of dental evolution, vii
- Half-century of trituberculy, the Cope-Osborn theory of dental evolution, William K. Gregory, vii
- Heilbrunn, L. V., The action of anæsthetics on living protoplasm, 159
- Henry, Jules, xvii
- Historical Society of Pennsylvania, vii
- Hookeri* Torrey and Gray, What is *Oenothera* (Davis), 245
- Hrdlička, Aleš, Normal variation, 253
- Huebner, S. S., Economic functions of the stock market as a business agency and the proposed National Securities Exchange Act of 1934, 77
- Impatiens*, Prochromosomes and chromosome structure in (Smith), 193
- Industrial recovery, Some subjective factors affecting (Wetherill), 129
- Jepsen, Glenn L., A revision of the American Apatemyidæ and the description of a new genus, *Sinclairiella*, from the White River Oligocene of South Dakota, 287
- Johnson, Douglas, Geomorphic investigations in the Yellowstone Park and Big Horn regions of Wyoming, xi
- Johnson, Emory R., Problems of government ownership and operation of the railroads, 103
- Johnson, Jotham, Excavations of the University Museum at Minturnæ, xxiii
- Kurath, Hans, The linguistic atlas of New England, 227
- Lafayette Medal, xvii
- Laws, revision of, viii, ix
- Leland, Waldo G., ix
- Lewis, John F., Jr., xxiii
- Liberty, property and recovery, James T. Young, 93.
- Linguistic atlas of New England, Hans Kurath, 227
- Livingston, Burton E., New developments in automatic control of soil moisture in plant cultures, x
- Logan, James, vii
- Long voyages of the Polynesians, Roland B. Dixon, 167
- MacCurdy, George Grant, New light on prehistoric man in Asia, 185
- Magellanic Medal, xii
- Mains, Edwin B., The genera *Cordyceps* and *Ophiocordyceps* in Michigan, 263
- Man in Asia, New light on prehistoric (MacCurdy), 185
- Meetings, Minutes of, vii
- Melanophores, Neurohumors as activating agents for fish (Parker), 177
- Members admitted:
- Chinard, Gilbert, xii
 - Collins, William H., xii
 - Cook, Gustavus Wynne, xxiii
 - Dinsmoor, William B., xii
 - Huntington, Edward V., xii
 - Jones, Lewis R., xii

- Kofoed, Charles A., xii
 Livingston, Burton E., ix
 Morgan, Marshall S., viii
 Murlin, John R., xii
 Packard, Francis R., vii
 Sanders, Henry A., ix
 Wetherill, Samuel Price, xiv
 Members deceased:
 Britton, Nathaniel Lord, xxii
 Cadwalader, John, xxii
 Cajal, Ramon y, xxii
 Curie, Marie Skłodowska, xxii
 Davis, Arthur P., vii
 Davis, William M., viii
 Gest, John M., xxiii
 Gilbert, Cass, xxii
 Hess, Alfred F., vii
 Macallum, Archibald B., ix
 Poincaré, Raymond, xxii
 Schuster, Sir Arthur, xxii
 Shorey, Paul, xxii
 Squier, George Owen, ix
 Trowbridge, Augustus, ix
 Welch, William H., xxii
 Wilson, James C., xxii
 Members elected:
 Bronk, Detlev W., xiii
 Cather, Willa, xiii
 Cook, Gustavus Wynne, xiii
 Cross, Wilbur L., xiii
 Gilbert, Cass, xiii
 Harkness, Edward S., xiii
 Jayne, Horace Howard Furness, xiii
 Kidder, Alfred Vincent, xiii
 Lowe, John Livingston, xiii
 Novy, Frederick, xiii
 Read, Conyers, xiii
 Reeves, Jesse S., xiv
 Roberts, Owen J., xiv
 Sarton, George, xiv
 Taylor, Deems, xiv
 Merriam, John C., Conservation and evolution in a changing social program, viii
 Migration into all parts of the world except Australia, The thirty-nine distinct lines of proboscidean descent, and their (Osborn), 273
 Miller, John A., xii
 Millikan, Robert A., xvi
 Minerals, The University of Michigan collections of antarctic rocks and (Stewart), 311
 Molecules in stellar atmospheres, Henry Norris Russell, xi
 Morris, Roland S., xii, xvi
 Murlin, John R., Some comparative results with a new semi-automatic respiration calorimeter, xi
 National planning, The economic implications of (Delano), 21
 National Securities Exchange Act of 1934, Economic functions of the stock market as a business agency and the proposed (Huebner), 77
 Neurohumors as activating agents for fish melanophores, G. H. Parker, 177
 New amplifications of the North American Piperaceæ, William Trelease, x
 New developments in automatic control of soil moisture in plant cultures, Burton E. Livingston, x
 New England, The linguistic atlas of (Kurath), 227
 New fishes from the Triassic of Pennsylvania, W. L. Bryant, xi
 New light on prehistoric man in Asia, George Grant MacCurdy, 185
 New synthesized pure-breeding chromosome type in the Jimson weed, *Datura stramonium*, A. F. Blakeslee, xv
 Nicholson, Blake E., Duplicate tax assessments, xvii
 Nominations pending, viii
 Normal variation, Aleš Hrdlička, 253
 Nuclear physics at the Bartol Research Foundation of the Franklin Institute, W. F. G. Swann, xi
Oenothera Hookeri Torrey and Gray, What is (Davis), 245
 Officers elected, xiii
 Oligocene of South Dakota, A revision of the American Apatemyidæ and the description of a new genus, *Sinclairiella*, from the White River (Jepsen), 287
Ophiocordyceps in Michigan, The genera *Cordyceps* and (Mains), 263
 Osborn, Henry Fairfield, The thirty-nine distinct lines of proboscidean descent, and their migration into all parts of the world except Australia, 273
 Parker, G. H., Neurohumors as activating agents for fish melanophores, 177
 Pathology, Some aspects of genetic constitution in relation to (Gowen), 215
 Patterson, Ernest Minor, Factors controlling prices, domestic and international, 63

- Pennsylvania Local Government Survey, The progress of the. James T. Young, xvii
- Penrose, R. A. F., Jr. Fund, Report of projects sponsored by, xvii
- Penrose, the R. A. F., Jr., Memorial Lecture, A generation's progress in the study of evolution, Edwin G. Conklin, 137
- Petrography of some rocks from South Victoria Land, Duncan Stewart, Jr., 307
- Phillips, Henry M., Prize, xii
- Planning, Esthetic and sociological aspects of city and regional (Grèber), 15
- Planning, The economic implications of national (Delano), 21
- Planning, Some governmental aspects of regional (Radcliffe), 1
- Poincaré, Raymond, xxiii
- Polynesians, The long voyages of the (Dixon), 167
- Prehistoric man in Asia, New light on (MacCurdy), 185
- Prices, domestic and international, Factors controlling (Patterson), 63
- Prince, John Dyneley, Turkish remains in modern Belgrade, xv mentioned, xvi
- Problems of government ownership and operation of the railroads, Emory R. Johnson, 103
- Proboscidean descent, The thirty-nine distinct lines of . . . and their migration into all parts of the world except Australia (Osborn), 273
- Prochromosomes and chromosome structure in *Impatiens*, Frank H. Smith, 193
- Properties of catalytic surfaces as revealed by heavy hydrogen, Hugh S. Taylor, x
- Property and recovery, Liberty (Young), 93
- Protoplasm, The action of anaesthetics on living (Heilbrunn), 159
- Radcliffe, George L., Some governmental aspects of regional planning, 1
- Railroads, Problems of government ownership and operation of the (Johnson), 103
- Recovery, Liberty, property and (Young), 93
- Recovery, Some subjective factors affecting industrial (Wetherill), 129
- Regional planning, Esthetic and sociological aspects of city and (Grèber), 15
- Regional planning, Some governmental aspects of (Radcliffe), 1
- Reorganization of the first class township, Phillip B. Willauer, xvii
- Reorganization of the second class township, Edward W. Carter, xvii
- Report on the White River fauna, William B. Scott, xi
- Revision of the American Apatemyidæ and the description of a new genus, *Sinclairella*, from the White River Oligocene of South Dakota. Glenn L. Jepsen, 287
- Rocks and minerals, The University of Michigan collections of antarctic (Stewart), 311
- Rocks from South Victoria Land, The petrography of some (Stewart), 307
- Russell, Henry Norris, Molecules in stellar atmospheres, xi
- Sanders, Henry A., The Beatty papyrus of the epistles of Paul, xv
- Scott, William B., The report on the White River fauna, xi
- Report on Philadelphia membership, ix
- Securities Exchange Act of 1934, Economic functions of the stock market as a business agency and the proposed National (Huebner), 77
- Simple, rapid basal metabolism measurement for hospital routine, Francis G. Benedict, xi
- Sinclairella*, from the White River Oligocene of South Dakota, A revision of the American Apatemyidæ and the description of a new genus (Jepsen), 287
- Smith, Frank H., Prochromosomes and chromosome structure in *Impatiens*, 193
- Society of American Bacteriologists, vii
- Sociological aspects of city and regional planning, Esthetic and (Grèber), 15
- Solar radiation, lightning, the azotobacter and protoplasm, George Crile, xi
- Some aspects of genetic constitution in relation to pathology, John W. Gowen, 215
- Some comparative results with a new semi-automatic respiration calorimeter, John R. Murlin, xi

- Some governmental aspects of regional planning, George L. Radcliffe, 1
- Some physico-chemical properties of the virus of typical tobacco mosaic, B. M. Duggar, x
- Some subjective factors affecting industrial recovery, Samuel Price Wetherill, 129
- Spectral survey of the nearest galaxy, the large magellanic cloud, Annie J. Cannon, xi
- Stabilization of the currency, Ray B. Westerfield, 29
- Stewart, Duncan, Jr., The petrography of some rocks from South Victoria Land, 327
- The University of Michigan collections of antarctic rocks and minerals, 311
- Stock market as a business agency and the proposed National Securities Exchange Act of 1934, Economic functions of the (Huebner), 77
- Swann, W. F. G., Nuclear physics at the Bartol Research Foundation of the Franklin Institute, xi
- Taylor, Hugh S., The properties of catalytic surfaces as revealed by heavy hydrogen, x
- Thirty-nine distinct lines of proboscidean descent, and their migration into all parts of the world except Australia, Henry Fairfield Osborn, 273
- Time and its determination, Ernest W. Brown, viii
- Torrey and Gray, What is *Oenothera Hookeri* (Davis), 245
- Trelease, William, New amplifications of the North American Piperaceae, x
- Turkish remains in modern Belgrade, John Dyneley Prince, xv
- University of Michigan collections of antarctic rocks and minerals (Stewart), 311
- Variation, normal (Hrdlička), 253
- West, Bradford W., Administrative problems of borough organizations, xvii
- Westerfield, Ray B., Stabilization of the currency, 29
- Wetherill, Samuel Price, Some subjective factors affecting industrial recovery, 129
- What is *Oenothera Hookeri* Torrey and Gray, Bradley Moore Davis, 245
- Willauer, Phillip B., reorganization of the first class township, xvii
- Young, James T., Liberty, property and recovery, 93
- The progress of the Pennsylvania local government survey, xvii

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